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REVIEWS OF MODERN METEOROLOGY—4

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The evolution of 20th-century forecasting in the British Isles*

By C. K. M. DOUGLAS
Meteorological Office, Dunstable

1. Introduction

During the present century there has been more or less continuous progress in synoptic meteorology, due primarily to the steady increase of observational material. Reports from ships in the Atlantic are an obvious necessity for satisfactory forecasting for the British Isles, and these began before the first world war. The first wireless reports from Atlantic liners were in 1909, and by 1914 they were quite numerous, but they ceased in the later part of the war and were not renewed till 1921. The exploration of the upper air began early in the century, and immediately led to wholly new ideas about the atmosphere, and ever since then there has been a steady growth in our knowledge of the three-dimensional atmospheric processes. Since the sounding balloons were not recovered till some days later they could not be used for forecasting, but the knowledge derived from them affected the interpretation of surface observations and influenced the ideas of the more advanced forecasters from the time of the first world war onwards to an increasing extent. The practical results of increased scientific understanding are difficult to determine. So far they have been rather disappointing, for reasons which I shall attempt to explain later.

The complexity of the atmosphere makes forecasting a very woolly subject unsuited to description in precise terms. It is impossible to give a detailed history of forecasting techniques, since they are inherently indescribable, and no adequate account of them has ever been published at any time in any country. The present essay only deals with some aspects of the subject, and is mostly based on personal memories. It deals chiefly with the development of scientific ideas which enter into the interpretation of synoptic charts, but some remarks will be made on their practical applications, limited though these are. No attempt is made to include a bibliography of the subject, which would have to be very long if it were at all comprehensive. A few publications are referred to but these are either recognized classics or else they are introduced incidentally owing to their relevance to the points discussed.

2. Forecasting before 1914

Progress throughout the century has been fairly continuous, but the observational advances and the changes in method were specially marked during the two world wars, owing to the demands of the Services. Both the war periods were exceptional and

* A review entitled 'Principles of synoptic weather forecasting' by R. C. Sutcliffe will appear subsequently in this series. (Ed.)

transitional. The three main epochs were before 1914, that between the wars, and the modern period which is only now slowly beginning to take shape. A chronological sequence will be followed, but without any rigid divisions, since a developing idea has to be seen in perspective, and should be assessed in the light of modern knowledge. The main sources of information about the pre-1914 period are Shaw's *Forecasting Weather*, published in 1911, and the *Daily Weather Reports* of that time, which contain forecasts for the 24 hours from noon. Shaw's great work, like all others on the subject, deals mainly with synoptic description, though there is a general outline of forecasting procedure. Since the book was intended for the general reader the elements of the subject had to be included, but there is also an account of the physical processes of weather and of the most recent work on trajectories and line-squalls. These portions of the book are of great historical importance in that they link up physical ideas with forecasting problems, but they could not have much immediate practical effect. The empirical methods described by Shaw are broadly the same as those in Abercrombie's *Weather*, which is highly praised. There is no doubt that Abercrombie did much for synoptic meteorology, and he certainly helped to give it the status of a science, though not of an exact science. Nevertheless the actual practice of forecasting at the beginning of this century was so completely empirical that its character must have been determined by the nature of the available material, and no one man, however able, can have appreciably affected it. Forecasting has always depended on the personal experience and judgment of the forecasters themselves in dealing with each situation as it arises, and this cannot be formulated as invariable rules, though there are certain guiding principles. It is impossible to put into any book just how the difference between good and bad forecasting arises. A good forecaster is one who is more likely to know when *not* to apply text-book rules, but his decision, often reached after much hesitation and worry, with the low confidence which comes with knowledge when the situation is really difficult, is based on many features on a particular set of charts. No problem is ever repeated in detail. The good forecaster will get better results on the average, but in a particular case he may be unlucky and regret that he did not follow the text-book. No comparison is possible between a forecast problem as seen at the time and when seen after the event.

Forecasting Weather only includes two complete sets of forecasts, one of them on a reproduced *Daily Weather Report*. Altogether 136 isobaric charts were included in the 1911 edition, many on a small scale. In two cases of unusual rainfall it was mentioned there was a forecast failure. In addition to this a number of charts of unusual interest are reproduced, illustrating gales, snowstorms and a striking sequence of developments in December 1909. Reference to the *Daily Weather Report* shows that none of the important developments were in the least foreseen 24 hours previously. Naturally the most striking and important developments are the most difficult to predict, and this is still true today.

Before the 1914 war the routine forecasts issued to the Press were regularly checked, in respect of wind direction, wind force, state of sky, precipitation, temperature, and other phenomena, if any. It is stated in *Forecasting Weather* that the average results were that 56 per cent were wholly successful and another 31 per cent more nearly right than wrong, so that ' 87 per cent gave a fairly accurate indication of weather for the period to which the forecasts refer.' It is stated that in the hands of experienced checkers the system worked mechanically, but that when the checker was inexperienced the figures tended to come out higher. It should be noted that in the absence of ' other

'henomena ' (snow, fog, thunder, etc.) the forecast was more right than wrong if wind nd temperature were correctly forecast, so that the system of checking was a lenient ne. When the 1914 war broke out the increased pressure of work on the small staff f assistants led to the checking being dropped, and it was never renewed.

The visual gale-warning system in operation in those days was the same as it is ow, apart from the increase in the number of places which fly cones. A ' missed ' ale was one which started less than 2 hr after a gale-warning message, if any, was lispatched. The following paragraph in *Forecasting Weather* is worth quoting, for he light that it throws on all progress in weather forecasting :

> It has always been supposed that the facilities for identifying the approach of a depression carrying winds of gale force would be so much facilitated by reports by radio-telegraphy from the Atlantic, that when these were once received, the avoidance of " missed " gales would be an easy matter. This view is doubtless based upon the assumption that gale-carrying depressions pass across from the western region of the Atlantic, the neighbourhood of the transition from warm to cold water, almost with the regularity of an ocean steamer. For that, as for so many other general views of meteorological conditions, some striking instances can be adduced, but it is, unfortunately, not generally true. Since January 1909, when the transmission of wireless messages from liners began, ... we find that we have still to watch with the same care, and with hardly less anxiety, the course of events off our western shores.

In 1951 hourly reports are received from our western coasts, and they frequently have to be constantly watched not only in connection with gale warnings, but for all kinds of forecasting.

When the ' further outlook ' was first introduced it was based on the general weather type prevailing and did not refer to a fixed 24-hour period commencing at the end of the period covered by the main forecast. When there is no marked temperature feature the main point which interests the public is whether the weather is settled, *i.e.*, fair, with no rain, or unsettled, that is changeable with no prolonged fair weather. Some years after the B.B.C. forecasts had started in 1924, the expression ' further outlook unsettled ' became a joke, but it had its origin in an attempt to convey to the public some idea of the weather type. This problem has not yet been solved.

The ' fine spell ' notifications for the hay and harvest periods were in operation early in the century. Shaw does not state the degree of success attained, but it is fairly certain that the forecasters waited till an anticyclone was established, so that there should have been a very fair measure of success.

The forecasters of that period attempted less than is done now, particularly as regards timing. If the wind or weather expected within 24 hours differed markedly from that prevailing at the time there was a broad division into ' at first ' and ' later,' but more often there was a general 24-hour forecast based on the pressure distribution. On the west coasts, where ample Atlantic information is most necessary for detailed forecasting, the persistence of type leads to a fair measure of success in general forecasting, as there are a good many spells of dry or wet weather. There was a spell of 32 days in the late autumn of 1911 with measurable rain (·01 in. or more) at Valentia, and the forecasts of rainfall were 100 per cent correct, since on every day either ' some rain ' or ' some showers ' were forecast. For checking purposes rain and showers could not be distinguished with the information then available, and even now it would sometimes be difficult. In our eastern districts rainfall forecasting is intrinsically more difficult than it is in the west, but if there are no ships' observations there is better material in the east for a wholly synoptic forecast as distinct from a partly climatological one. On the average it takes rain or clearing weather over 12 hours to travel from Valentia to London, and if a short forecast for Valentia can be deduced from the local observa-

tions there is material for a successful 24-hour forecast for London if events proceed in a normal way. In the absence of Atlantic information, forecasting often resolves itself into a study of the latest observations from our western coasts, especially barometric tendency, wind and cloud. Before 1914 only the general degree of cloudiness was reported, but in some situations, such as a wedge, increasing cloud usually means an approaching depression, though it could easily be misleading. Lack of observations of cloud forms was a gap in the pre-1914 synoptic information.

Forecasting thunderstorms was one of the most successful branches of pre-1914 forecasting, as it did not often depend on information from ships. At first sight it may seem surprising that much could be done without upper-air observations, but there is a fair degree of correlation between upper-air and sea-level conditions. Thunderstorms are fairly certain if a depression spreads up from France or the Bay of Biscay, and falling tendencies generally give warning of it. Other easily recognizable thundery situations are a trough of low pressure moving east with very warm southerly winds ahead of it, a ' col ' in warm weather, and the cyclonic showery type of cool weather. In some flat or complex situations there is a difficulty, but there is sometimes a long series of thunderstorm days and then only the first day presents any difficulty. Most of the main thundery outbreaks were forecast.

The four forecasters of that period were men who had started in the Forecast Division as assistants and had a very long experience of charts. For the forecasting of that period experience was more important than science, and its value was shown by the able manner in which the less frequent developments, such as the spreading back of depressions or rain from the Continent, were sometimes handled. It is interesting to note that these forecasters had no belief in the value of individual past cases or ' analogues ' as a guide to forecasting. It was said that as far back as the 1890's this method had been tried and found wanting, and subsequent attempts have confirmed this conclusion. The experience on which the individual forecaster builds up his technique is based on the normal modes of behaviour shown in a large number of somewhat similar situations. The abnormal case is never repeated. In spite of some good individual forecasts, it would be difficult to prove that the pre-1914 forecasts for 24 hr periods were on the whole better than could have been attained by statistical methods, based on persistence with some reversion to the normal. By ' persistence ' is meant recent trends and not merely a repetition of today's weather. If there is one dry day in the middle of a wet spell forecasts based on mere repetition would contain two errors, but fully developed statistical methods would in some cases indicate a continued probability of rain, especially in the west with winds from any westerly point, so that there would only be one error. The pre-1914 forecasting organization should not be judged solely by the forecasts produced but also by what it afterwards developed into, particularly in the 1914-1918 war, and this principle should be kept in mind whenever comparisons are made between synoptic and statistical methods of forecasting.

3. Forecasting in the first world war

The outbreak of the first world war soon led to a greatly increased demand for meteorological information and forecasts, and the British Expeditionary Force in France and the Navy both developed meteorological services. The effect of temperature and wind on shell trajectories was the most immediate application, but there was also

important and increasing demand for forecasts. The close contact between the eteorologists and the users of the forecasts led to the development of what was largely new type of forecast, namely the detailed short-range forecast. Previously there had en little demand for these, since it was impossible to disseminate them to a wide ıblic, and for the minority vitally interested in the weather the necessity was not mparable to that of war-time. It is interesting to refer to a passage in Shaw's *Fore-sting Weather* which describes a case of outbreaks of rain extending as far west as eland, associated with a NNE. current over the British Isles and a depression over st France. Shaw quotes an extract from C. J. P. Cave's weather diary which describes e movement of thunderstorms from the SE., with scud below from the N. and adds, uch rain is beyond the forecaster's horizon.' It was beyond the horizon of the fore-sting then carried out in London, but not beyond the horizon of Cave and other rst-class observers of clouds and weather. There are obvious limitations to single-bserver forecasting, but unless the sky is overcast with thick low cloud it is often ossible to foresee the weather for a few hours ahead, and sometimes for the whole ıy. Clearly the best results are obtained by combining synoptic methods with local bservations, and even with the wealth of information available in modern times the udy of the sky is still a most useful aid to short-period forecasting for a limited area. his does not mean only one place, since the range of vision of clouds often exceeds) mi and may exceed 100 mi, and the chart sometimes shows that local conditions are epresentative of half England.

The existing Meteorological Office organization provided the essential basis for l the war-time forecasting. A collective synoptic message was telegraphed to the ritish Expeditionary Force. A new code was introduced which included observations f visibility, cloud types, height of low cloud, and much more detail about present eather. At that time only selected stations, mostly with professional observers, used ıe new code. After the war it was made general, but observations of clouds have ways presented difficulties to part-time observers. There never has been a time hen all the cloud observations could be accepted at their face value, but forecasters ıon get to know which stations can be relied on. The new information naturally ffected forecasting, and physical ideas increasingly entered into it, including what ere later called ' air mass ' ideas. The work of Shaw and Lempfert on trajectories nd their emphasis on currents of air paved the way for this. Observations of tempera-ıre and humidity from kite balloons and later from aircraft, obtained primarily for rtillery ' meteor ' corrections, enabled the treatment of fog and low clouds to be laced on a three-dimensional basis. The ' Meteor Flight ' at Berck was started early ı 1918, and soundings up to 14,000 ft became available for forecasting for the first me. Hertz diagrams were used for forecasting thunderstorms, showers, and cloud ructures. There were pilot-balloon observations on a large scale and the term ' thermal ind ' was in routine use. As the war progressed, forecasts became increasingly ecessary to air pilots. In its earlier stages flying was only carried out in good weather, ut later on weather risks had to be accepted, especially for night bombers and for the ndependent Air Force with its flights into Germany, which were long for those days. During the whole war 24-hour forecasts and further outlooks were sometimes of great nportance for planning purposes. The 0100 chart (1915), observations at supple-ıentary hours, and round-the-clock rosters date from this time.

During this period the war-time needs of a small neutral country, Norway, largely ependent on agriculture and fishing, led to a development of great importance in the

history of forecasting. The first discoveries were based on a close network of observations but later developments did not depend on this. They will be the main subject of discussion in dealing with the inter-war period. The Norwegian work was often referred to as the ' Bjerknes theory ' but the word ' theory ' is unsuitable for anything in synoptic meteorology. It is really a technique based on simplified models of atmospheric structure and movement, and its successes have been based on its empirical rather than its theoretical aspects.

4. The inter-war period – the Norwegian methods

After the first war the Royal Air Force was cut down to an extent which later events proved to have been unwise, and aviation forecasting only continued on a small scale, its most important application being on the civil air routes from Croydon to Paris and Brussels. Later on there was an expansion both of military and civil aviation. It was only aviation forecasting which could be made detailed. Broadcast forecasts by the B.B.C. started in 1924, but for several years they were only issued at 6 p.m. and 9 p.m., and the most important part of them referred to the following day, so that they could not be made detailed even if there had been room for it, which there was not. A few years later land-area forecasts were also issued in the morning and more detail could be given. The Norwegian technique is specially suitable for detailed forecasts for the first twelve hours. When the time interval is extended to 30 hours from the last completely analysed chart, as with most Press and B.B.C. forecasts, the difficulties of timing are great and a high degree of precision is impossible, whatever technique is used.

It is assumed that the main features of the Norwegian ideas are generally known, and it is only proposed to discuss certain points about which there was some controversy and perhaps some misunderstanding. The widespread adoption of the technique was due to its suitability for surface charts, which were the only ones available, but the underlying ideas were essentially three-dimensional. The emphasis on rainfall and the occlusion process meant that the ascent of air was fundamental, and subsidence and dynamical warming, the importance of which had previously been emphasized by W. H. Dines, were soon brought into the picture. The interest of the leaders of the Norwegian school was always centred on the main developments, rather than on the details of aviation forecasting. It was soon realised that the frontal discontinuity is often smoother in the upper air than it is near the ground, but this was not considered important. Bergeron talked in terms of isosteric-isobaric solenoids (following V. Bjerknes), and this is the same as a baroclinic zone. Indeed the very terms ' barotropic ' and ' baroclinic ' were introduced by V. Bjerknes. The methods were well publicized and by some subtle process exaggerated hopes came into being, though these cannot be traced to any definite statement by any of the leading members of that school. As a reaction against this there was some criticism of excessive claims and over-simplification. Some over-simplification and unwarranted optimism there certainly was, but without these it is doubtful whether so much valuable research would have been done. Sir George Simpson once remarked that the Norwegians had infused new life into synoptic meteorology. The enthusiasm was primarily directed to an increased understanding of the atmosphere, but there was also an underlying assumption that forecasting would benefit to a substantial degree. This has been partially justified, and more than this cannot be said of anything in the whole history of forecasting.

In the summer of 1920 Prof. V. Bjerknes and Helland Hansen invited Sir Napier Shaw to visit Bergen with a small delegation of British meteorologists to discuss the new ideas. The three other meteorologists were Dr. A. H. R. Goldie, Dr. L. F. Richardson and the present writer, who will all remember the enthusiasm which existed at Bergen. The Norwegians present included V. and J. Bjerknes, Bergeron, Rossby, Hesselberg, Solberg, and Calwagen, who was afterwards killed on a meteorological flight. Prof. F. M. Exner of Vienna also attended the latter part of the meeting. The Norwegians were entirely open-minded and willing to recognize that the atmosphere was complex, which was certainly the opinion of their visitors. Dr. Richardson was about to publish his book *Weather prediction by numerical process*, and Sir Napier Shaw mentioned that Prof. Bjerknes and Dr. Richardson were the only men engaged on a direct attack on the main atmospheric problem. Dr. Richardson certainly thought the problem highly complex both in its theoretical and practical aspects. I showed slides of cloud photographs and of temperature fluctuations at Berck which had a bearing on fronts and associated clouds, and described both some typical altostratus-nimbostratus cloud structures and also complex multi-layer types, an account of which had been published. Not long afterwards Dr. J. Bjerknes visited Paris to co-ordinate his views with the French ideas on travelling *systèmes nuageux* and the related isallobaric lows. These included altocumulus systems which were attributed to weak fronts, and the *ciel de marge* often noted in France at the southern flank of rain areas was explained in the same way in many cases. The reason why the Norwegians adhered to the thick mass of cloud in their main warm-front model was that to them the most typical warm front was well within a deepening depression, with rapidly falling pressure and substantial rain ahead of it. The question of the frequency of different structures had to await further observations, and even now the observational material is inadequate for statistical purposes.

During the 1920 meeting Prof. Bjerknes showed a slide of Fig. 96 of Shaw's *Forecasting Weather* (1911 Edition) which foreshadowed the Norwegian model of a depression. Shaw remarked that he had come to regard these features as largely superficial and that he looked on depressions as vortices, mentioning Dines's correlation coefficients in support of this view. Bjerknes replied that in his opinion the vorticity was concentrated along the fronts in the earlier stage of development and afterwards spread over the depression. There is a good deal of truth in this, but the details of the process are still not fully understood. In the light of later developments it may well be that the Norwegians would have done better to have paid less attention to surfaces of discontinuity and more to the generalized problem of divergence and vorticity, but this would only have affected the work of a few mathematicians. It could not have influenced practical developments, since vorticity could not be applied to the main three-dimensional problem till upper-air charts were available. Later on J. Bjerknes took a leading part in the development of the newer outlook, and in one of his papers (Bjerknes 1932) included the first dynamical relationship between upper divergence and the flow round an upper trough. The precise status of the surface of discontinuity is still in doubt. Some young depressions have such a surface extending to the tropopause, though with complexities, but others have not, and there is no obvious difference of behaviour. This is the main reason for the modern preference for the baroclinic zone.

The Norwegian school attached much importance to the ideas of Margules on the conversion of some small part of the potential and thermal energy of the atmosphere

into kinetic energy. Margules worked with highly idealized models but he introduced the principle that the vertical circulation with suitable horizontal temperature differences and lapse rates releases the kinetic energy required for the horizontal motion. Gravitational and thermal energy are both drawn on as they change together, owing to the elementary fact that when a column of air is warmed its centre of gravity rises. As Sir Charles Normand (1937) has pointed out, he obscured the importance of the latent heat of condensation by only considering cases when a complete spreading of the cold air under the warm could take place without condensation. If in such cases rising saturated air in neutral equilibrium is substituted for rising dry air, also in neutral equilibrium, the condensation merely warms up the top layer without adding to the kinetic energy. In the conditions actually observed in the atmosphere only a partial overturning takes place, and the condensation is vitally important, since without it the amount of vertical circulation would be small. If the rising warm air is convectively unstable kinetic energy is released in it as irregular motion. The general warming of the upper air in the unstable region may give rise to a horizontal temperature gradient and so to a new source of energy, but the scale of this is usually small in our area. The writer attributed importance to vertical instability as a factor in the development of polar air depressions by 1920. The Norwegian school as a whole attributed great importance to the release of latent heat long before Refsdal (1929) published his valuable paper on *Der Feuchtlabile Niederschlag* which attached too much importance to instability in relation to the deepening of occluded depressions. There is now plenty of observational material which shows that post-occlusion deepening is due in the main to horizontal gradients of temperature, and this also holds for many polar-air depressions, though instability is a supplementary factor in some cases. The modern view of the role of instability was well summarized by E. J. Sumner (1950).

The latent heat makes the whole problem difficult. A recent paper by J. Spar (1950) showed that there was no decrease of potential energy over an area covered by a deepening depression and its periphery, and he regards this as evidence against Margules, but this is quite normal, and since latent heat was ignored the conclusion does not follow. Condensation reduces the rate of cooling of rising air and the rise of temperature shows up in the descending air, as in the Föhn process. In the long run this is compensated by outward radiation. In discussing the energy of the upper currents a very large area would have to be considered, but in the lower troposphere there is rapid development of kinetic energy during cyclogenesis, and the only available source is the Margules type of process acting through the pressure gradients. The release of energy implies that the rising air is warmer on the whole than the descending air. This very important point is obscured if the formation of a cold depression is attributed primarily to ascent of air. The coldest part of the developing depression is the rear portion where the air is usually descending, and the ascending saturated air can rarely become so cold till it has been cooled by radiation.

Modern knowledge confirms the Norwegian view that thermal advection is of primary importance, though advective changes are counteracted by other factors and heavily damped down. J. M. Craddock (1951) has treated the subject in a quantitative manner, using thicknesses from 1,000 to 700 and 700 to 500 mb., which are proportional to the mean temperatures of the layers at the place considered (Downham Market). The advective changes are correlated positively with the actual changes and negatively with the non-advective change. In the neighbourhood of the British Isles, where the gradient of sea temperature is low, the vertical movement is usually the

ost important non-advective factor, so that Craddock's figures fit in with the view at ascending air is on the whole warmer than descending air.

Bergeron (1928) published a classical paper on three-dimensional analysis which cluded a discussion of frontogenesis. The isotherms are drawn together in the st place by deformation of the air mass in suitable isobaric distribution, the ' hyper-olic ' flow pattern being the ideal one. The frontal zone is a product of the previously isting depressions and anticylones. A long, fairly straight front is usually a trail t by a moving depression, which can be occluded at the centre, and is best defined nen there is a warm anticyclone to S. or SE. Convergence is the main agency in arpening the front. More recently Sutcliffe has suggested that it is only convergence hich causes undercutting of the cold air and therefore an approximate surface of scontinuity, at least low down. It is difficult to see how the undercutting could take ace without a vertical circulation and ageostrophic motion, of the type which is set o when the horizontal temperature gradient is increasing. Moreover for statical asons a sloping boundary surface with the cold air below requires cyclonic shear of e geostrophic wind at the front, and this can only be produced by convergence. The cent discoveries about the relation of the ' jet stream ' to the frontal surface support is view. It is not clear why some frontal surfaces extend right up to the stratosphere, ut this may require rather special conditions, such as a high wet-bulb potential mperature in the warm air and a really low temperature in the upper troposphere in e cold air. Since convergence is fundamental in producing a real front rather than frontal (or baroclinic) zone, a line of rainfall normally represents a real front when it more or less parallel to the isotherms in the lower troposphere. For reasons given elow, restraint should be exercised regarding the number of fronts put on the large-ea working charts.

5. Practical applications of the Norwegian ideas

The now world-wide system of frontal terminology and of marking-in fronts in olour on working charts and in black and white on published charts was largely the ork of J. Bjerknes, who has probably had more influence on synoptic meteorology an any other individual. Its continued world-wide use is itself a proof of its value, ut against the undoubted gain one has to set the disadvantage of a large increase in e subjective element on a synoptic chart. There never was complete objectivity in e drawing of the isobars, but this defect could be minimized by having enough bservations. Before the days of fronts the isobars were drawn too smoothly, and terwards the frontal discontinuities have often been overdone, especially in areas ith few observations. Fronts were not published in the *Daily Weather Report* till 933, and there was good reason for this caution, since before that time there was o generally high standard of frontal analysis. Some years earlier a number of countries arted publishing fronts, and there was vast diversity between them. A great deal of is was due to sheer bad analysis, which indicated a marked degree of backwardness the standard of synoptic meteorology. J. Bjerknes informed me that some of the ublished monstrosities which were supposed to be fronts made him quite ashamed, he felt some responsibility owing to his international propaganda in favour of marking onts on the charts. (Dr. Bjerknes was for some months attached to the Meteorological ffice in 1925-26, and again in 1935-36). In the last twenty years there has been a otable improvement, due to better training and to the great increase of upper-air formation. Nevertheless it has been clear for some time past that even when avoidable

errors are eliminated, complete objectivity cannot be obtained in frontal analysis though the subjective element can be minimized if the number of fronts is kept fairly low. If however only the very obvious and pronounced fronts were put on, analysis would lose much of its value. Nowadays the upper-air thickness charts show up the general thermal distribution, but it is important to mark in a front where there is an appreciable chance of weather phenomena of frontal type.

Like so much in synoptic meteorology, fronts defy precise definition. Significant fronts vary greatly in structure and in the magnitude and degree of sharpness of the temperature and wind discontinuities. The part of the thermal field that can be called discontinuous is often embedded in a general gradient. Measurement along a fixed length across the front does not solve the problem, since there is no length that would be suitable for more than a limited group of fronts.

For a long time past it has been evident that the three standard types of front do not cover all cases, and in particular that any long series of analysed charts contains more occlusions than are formed by the orthodox process. The term ' pseudo-occlusion ' has often been used to denote fronts with the structure of occlusions but formed without definite pre-existing warm and cold front. When there is convergence and ascent of relatively warm air, the only variation from the normal process is that the warm front, and sometimes also the cold front, are not sufficiently well marked to go on the chart. Sometimes also a belt of rain develops in unstable air and a weak warm tongue is formed over or just ahead of it. (Old occlusions are sometimes quite close to the axis of a cold tongue even when formed originally by the normal process). Some other fronts are not easily fitted into the standard scheme owing to a reversal of temperature difference with height, and it would be an advantage to have ' unspecified front ' recognized internationally, and it is hoped that it soon will be. At the time of writing the old scheme is being used, apart from the introduction into current practice of ' warm occlusion ' and ' cold occlusion ' when there is a marked difference of air-mass temperature on the two sides of the occlusion. The terms themselves are old ones. The sending out of the frontal positions in code to out-stations started before the outbreak of war and was fully developed early in the war. As regards the number of fronts put on the working charts and the *Daily Weather Report*, the policy from 1933 to the present day has remained unchanged and accords well with the widespread modern view as to the kind of frontal analysis which a Central Forecasting Office should carry out. There are also minor fronts which are shorter in length and duration than the main ones. There may be advantages in putting these on the chart for very short-range forecasting, but not on the main charts of a Central Forecasting Office. The analyses in the *Daily Weather Report* (International Section up to 1949) are the only published 6-hourly series in existence.

The main practical application of fronts to forecasting is in respect of precipitation for all types of forecast, and also of clouds, which are specially important for aviation forecasting. Their value is greatest when timing can usually be made fairly accurate, especially the first 12 to 18 hours. There are also forecasting problems, especially of clouds, which depend on the study of air masses. The term ' air mass ' is the necessary complement of ' front,' and it can be applied to any region of the atmosphere not containing a front or frontal zone. In the Norwegian scheme fronts were primary, and air masses secondary. The Norwegians classified air masses, and the terms they introduced are still useful for descriptive purposes, but in our area ' maritime polar air ' is too large a class and there is no agreed system of sub-dividing it. Air-mass

meteorology is the analysis of the physical properties of the air away from the fronts. The moisture distribution is of primary importance but this is of course greatly influenced by temperature distribution. It is only the elementary part of the subject which is capable of formulation in general terms, and this is set out in introductory text-books. Apart from the names used the subject was never specifically Norwegian and the useful parts of it were developed independently in various countries, including our own. Its scope is very limited. The atmosphere is never really homogeneous, and though away from fronts or developmental areas the vertical motion is small in absolute magnitude, it is often large enough to affect the stability and other properties of the atmosphere in 24 hr. Air-mass analysis was directed mainly to estimating the upper-air structure from sea-level observations, and now that the upper-air observations are available, classification has lost its analytical and prognostic value. Measured quantities belong to a more exact and therefore higher scientific category than classified entities. This does not mean that the forecasting of clouds and other air-mass characteristics can yet take the form of precise computations, since as with other forecast problems there is vast complexity and only a limited time available. Each locality has its special problems, and there should still be scope for some empirical investigation on a local basis, but this should deal with the observations directly, without troubling much about the labels to be attached to the air masses.

We have already noted the difficulty arising from accurate timing of fronts, more especially in forecasts extending beyond 12 to 18 hours from the time of the chart on which they are based, which includes all forecasts covering the following day. This is the most constantly recurring of all the major forecast problems. It depends mainly on obtaining an accurate forecast of the pressure distribution, which is the key to nearly all 24-hour forecasting, and this in turn depends mainly on the movement and development of pressure systems. There is of course a mutual relationship between the motion of air masses and the development and movements of the systems into which the air motion is organized, but in practice it is the systems which are primary. Any notion to the contrary has never had any foundation either in forecast practice or in the ideas which emanated from Norway. In the inter-war period these ideas were of real help in estimating the movement and deepening of depressions in a limited number of cases. An important generalization was that warm-sector depressions move in the direction of the isobars in the warm sector. Another was that when a warm-sector depression has started to deepen the process normally continues, often at an increased rate, till after occlusion. A third was that when a depression becomes occluded its motion decreases and it may become stationary, at least for a time. Frontal analysis also showed where a new wave depression was most likely to form, and this was of some use even though one had to wait for a more definite indication, particularly in the barometric tendency, before basing one's whole forecast on it. In the aggregate the value of these principles was appreciable, but the number of good cases was not large. This was partly because most depressions are occluded when they reach the British Isles. At about the time of occlusion depressions tend to change their speed and track, and this is one reason why forecasting for the British Isles is specially difficult. This originates in geography and its influence on the temperature distribution, which leads to many difficulties besides the one mentioned above. When it was impossible to apply Norwegian principles, the forecasting of the pressure distribution depended entirely on the extrapolation of existing trends, modified as necessary on the basis of general experience, and was thus similar in essentials to the pre-1914 practice.

6. The introduction of the tephigram

Observations from aircraft were very irregular till a Meteor Flight was started in 1925 at Duxford. This was too far east and it was not till 1937 that another flight was started at Aldergrove. Shaw and Fahmy (1925) published an important paper on the tephigram, and it was introduced as soon as possible by J. S. Dines for the routine plotting of the Duxford ascents. The fact that the pressure lines cut the saturated adiabatics almost at right angles makes this diagram very suitable for showing up small variations of stability, and it was a great improvement on anything previously used in this country. It is widely used in the British Commonwealth, but most other countries use a version of the Neuhoff diagram. There is of course no difference in principle, and there is a natural tendency to adhere to the diagram in use unless there is a very good reason to change it. In this country the tephigram has developed into a major forecasting tool, used in various ways. Its original and most important purpose is in connection with vertical stability, especially of saturated air. The liability of the cumulus cloud to sustained vertical growth results from its excess of temperature and (to a much less extent) of humidity over that of its environment, and if the growth extends high enough, showers and often thunderstorms develop. The thunderstorms are certain if there is marked instability. The comparison of a mass of air rising adiabatically with an observed environmental curve, assumed undisturbed, has been called the 'parcel' method. J. Bjerknes and others have examined the effects of downward motion between the clouds, which must be taken into account in any complete discussion of the subject. No doubt it affects the number of storms which can form in a large area for any given value of the mean vertical motion over the area, which generally differs from zero, though there is no way of measuring it. The majority of thunderstorms form in a region of convergence, sometimes on a narrow belt of convergence, but a limited number form in areas which are neutral as regards cyclonic vorticity or its change with time, and in rare cases there is weak anticyclonic vorticity over the area as a whole, though not close to the developing storm. If there is no general convergence and the lapse rate is along the saturated adiabatic, any thunderstorms will be well scattered, but they may nevertheless occur. Unless there is clear evidence of continuing subsidence, the saturated adiabatic has to be regarded as the criterion for the stability of saturated air. There are many marginal cases with a lapse rate almost exactly saturated adiabatic, and there are small-scale variations in temperature and more especially in humidity, so that a vertical sounding does not always give a decisive indication for or against thunder or showers. Even in more definite situations the validity as regards time and area is often very limited. In the inter-war period the soundings were sometimes of value in forecasting thunderstorms for the same day, especially after the Aldergrove flight started in 1937. Some optimists thought that a good network of upper-air observations extending into the Atlantic would lead to reliable thunderstorm forecasting for the following day, but as we shall see later this hope has not been fulfilled. The use of the tephigram has been greatly facilitated by the application of Sir Charles Normand's ideas on the wet-bulb potential temperature, which were introduced at the instance of Sir David Brunt within a year or two of the production of the tephigram itself. The humidity has to be averaged over a layer about 100 mb thick, and is taken into account over a greater thickness, though there is no precise rule. The first outbreak of thunderstorms in a hot spell is often associated with upward growth from a cloud base between 900 and 850 mb into initially dry air, along a line of convergence. Recent American ideas about 'entrainment' merely

confirm the previous practice of taking humidity up to 700 or 650 mb into account in a vague manner. Two separate processes are involved in entrainment, as discussed by Prof. Byers, one the mixing process and the other the convergence into the lower part of a large mass of cumulus or cumulonimbus, up to a height well above the base.

7. Other advances in the inter-war period

Apart from the short-term use of the tephigram, the application of upper-air information to forecasting in the British Isles in the inter-war period was severely limited by geography. Upper winds up to considerable heights were occasionally available, especially in summer, and these were supplemented by nephoscope observations of medium and high cloud. These sometimes helped in analysis, especially when a strong NW. cirrus motion at Valentia indicated an approaching warm front. They were also sometimes useful in forecasting the movements of rain and thunderstorms, especially in summer.

Empirical ideas about the effect of upper currents on the movement of pressure systems have a long history. In the early pioneering days it was seen that high clouds were carried ahead of the depression and this was vaguely related to its movement. The application of three-kilometre winds to the movement of small isallobaric systems was included in the publication *Les Systèmes Nuageux* by Schereschewsky and Wehrlé (1923). Later the Germans followed a similar line but used the term ' stratospheric steering.' For quite a long time the dominant German academic school was obsessed with the idea that the stratosphere controlled the main atmospheric movements, since the ' centres of action,' *i.e.*, the warm anticyclones and cold depressions, extend to the stratosphere. In effect this treats the problem as one of statics and assumes that the tail wags the dog, and is itself controlled by some unknown factor such as high-level radiation effects. When the increasing demands of aviation led to a more synoptic outlook in Germany their ideas came nearer to earth, but there was no dynamical explanation of steering till Sutcliffe's (1939) paper. In this country geography made it difficult to apply even empirical upper-air steering ideas, but it was possible in a few cases.

In the 1930's Germany developed seven or more meteorological aircraft stations, and they played a pioneer part in developing upper-air charts, especially of the 500 mb absolute and relative topography, which we now call contours and thicknesses (this method of treating upper-air soundings was advocated by V. Bjerknes as early as 1910). Since they also received observations to westward they were well placed for the purpose. Their best-known work is that of Scherhag on what is now called difluence and confluence, but what he called divergence and convergence. He gave no dynamical explanation, and his best cases of falling sea-level pressure with difluent areas on the upper-air charts were due to the association of both with other factors. Nevertheless he was the first to notice the importance of these features. There has been a tendency to under-emphasize the acceleration along the trajectory in comparison with curvature effects, especially at high levels. Actually it may be equally important, though it works in a more complex way than Scherhag's earlier work appeared to suggest.

The paper by Brunt and Douglas (1928) brought out the importance of the acceleration along the trajectory at low levels (where the isallobaric term generally exceeds

the acceleration along the stream lines) in relation to frontal and non-frontal rainfall, the ageostrophic motion of warm and quasi-stationary fronts, and subsidence. It could not be applied numerically but it is reasonable to hope that a better understanding of the dynamical process associated with rainfall was of some use for empirical forecasting, even if there was no way of assessing what this amounted to, or even of proving its existence. This applies to other modern advances, which in the aggregate have now left 1928 far behind. There has been some progress in practical forecasting, but it is impossible to decide how much of it is due to an increase of information and how much to a better scientific understanding. In the decade before the second world war there was a substantial advance made in understanding the atmosphere, and in particular the papers published in the 1930's by J. Bjerknes, Scherhag, and Sutcliffe had outlined the framework in which the modern advance in three-dimensional synoptic meteorology has taken place.

8. From 1939 to 1946

The second world war, like the first, was an exceptional period, when forecasting was handicapped by lack of information from the Atlantic but nevertheless progressed in other ways. The first war initiated detailed short-period forecasting. In the second war this was greatly developed and expanded, but though very important in itself it was based on existing methods and had little permanent effect on forecasts as distinct from communications. The big advance was the development of the radio-sounding balloons and in particular of wind observations, leading to a new high-level aerology with an important direct application to high flying. Though the observational methods were worked out before the war, the scale of the operational development of the observations was greatly speeded up by war-time demands. The first radio wind-sounding to reach forecasters was made at Larkhill, in late October 1939, through frontal cloud in a very strong baroclinic zone, and obviously it opened a new era in synoptic meteorology, though its full implications could only be guessed at and are in fact still doubtful. Previously no wind observations had ever been made inside a cloud. (Some observations had been made above clouds in 1918-19 by flying through a series of smoke-shells fired from the ground). High flying also led to a demand for forecasts of cloud and icing at high levels. The only information available was that brought back by the aircraft themselves, which was linked up with other meteorological features for future forecasting purposes.

A combination of better upper-air information and a close network of hourly-reporting stations, with no Atlantic information, led to a new situation, and obviously there was a net loss except for very short periods ahead. The upper winds sometimes helped in the analysis, especially a strong NW. wind ahead of a warm front. There was a certain amount of empirical application of the steering principle and of the movements of rain and thunderstorms at middle heights, especially at 3,000 m (700 mb). Isobaric charts for 3,000 and 5,000 m appear in the Upper-Air Section of the *Daily Weather Report* for 1940. The German contour and thickness technique was introduced on 1 Jan. 1941, at the instance of Mr. E. Gold. The *Daily Weather Report* included 500 mb contours and 1,000-500 mb thicknesses over a limited area from that day, extended later to other isobaric surfaces. From Jan. 1942 there are published contour and thickness charts for 750, 500 and 250 mb (later changed to 700, 500 and 300 mb) and also a chart of tropopause height. Though only one set of charts per day was published, the working charts of contours and thicknesses were drawn three times

ily, becoming four daily later in the war, primarily for forecasting upper winds. he work was extensively developed under Dr. S. Petterssen in 1943-4. From 1941 ere were reconnaissance flights in the Atlantic, and from June 1944 also ascents om fixed ships. This led to cross-Atlantic upper-air charts (published from Sept. 943) and foreshadowed the modern period, but the fixed ships were withdrawn late 1945 and were not replaced till late in 1947.

The forecasts for the Royal Air Force were supplied by the out-stations. The fficial Meteorological Office history contains a record of the immense range of activity f the war-time forecasting organization at home and abroad. The great bulk of it was f detailed short-period type and many forecasters contributed to its success. The entral Forecasting Office supplied general guidance on the analysis and the expected evelopments, and from February 1942 this was greatly helped by the issue of forecast r ' prebaratic ' charts. The term ' baratic ' was already used for the coded messages iving pressure distribution on the actual chart. This term and prebaratic were intro- uced by Mr. E. Gold, who initiated these issues to out-stations. The coded message can ive a good representation if the coding is well done, *i.e.* if a good set of points on the sobars and fronts is chosen. When they were first introduced the time interval was 8 hours from the last main chart, or 13·5 hours from the time of distribution in coded orm. The introduction of the prebaratic as a routine was an important landmark in the istory of British forecasting. Previously they had only been drawn experimentally. There was a fairly prolonged experiment about 1925-7, largely confined to isobars ear the British Isles, but the results were not good. There is no doubt that good progress was made between then and 1942, particularly with regard to forecasting he movement of fronts. From the point of view of the actual forecast, especially of precipitation, the front is the most important thing on the prebaratic, but since its motion depends chiefly on the component of geostrophic wind at right angles to it, good forecast of the position of a front requires a good forecast of the pressure dis- ribution, especially since the time interval was extended to 24 hours in November 1946. The prebaratic represents the fullest development of the Norwegian methods of fore- casting, but nevertheless the most important part of its construction depends mainly on extrapolation of the development of the pressure field, which has always been the chief basis of forecasting. We shall see later that the most recent advances are modi- fying this situation to some extent, and that pure extrapolation is supplemented by deductions based on dynamical reasoning.

Some statistics of the mean errors of frontal positions on 18-hour prebaratics are given in the *Meteorological Office Professional Notes* No. 93. The mean error is about 90 mi, but there are large month-to-month variations, according as the month is a disturbed one or relatively quiet. There was a slight decrease in the mean errors in 1945 followed by a slight increase in 1946, due to the loss of the fixed ships and perhaps partly to the unsettled character of the summer. For 24-hour periods the error is about 120 mi, the increase being proportional to the time interval. It is too early to estimate the trend, as there are casual variations even in a period as long as a year, but it is reasonable to hope for a slight decrease in the errors of frontal positions, and of pressures and the positions and intensities of centres.

An error of 120 mi in distance corresponds to about 4 hr in time. This means that in a forecast for the following day it is difficult to give the public the full benefit of the main advance that has been made, which consists of the substitution of a timed sequence for a more general type of forecast. The existence of an appreciable public

demand for really accurate forecasts was shown by the popularity of 'Airmet' durin its limited period of operation, among people of widely different occupations.

9. Comparison with the position in U.S.A. in 1946

A good summary of the position in the United States at the end of 1946, most c it valid here also, was made by Prof. H. G. Houghton in his Presidential Address t the American Meteorological Society in December 1946 (published 1948). In th extract quoted below, I should be inclined to modify the first sentence and lay the stres not so much on our limited understanding of the physical processes as on the limitec prognostic application even of a fair degree of understanding.

The following is the extract :

> Our physical understanding of atmospheric processes is so limited that it is of little utility i weather forecasting. Consequently a forecast is made by the extrapolation of the principal pressur systems in accordance with past trends, as evidenced by previous maps and the pressure-tendenc field. To this is added the experience factor which is the fusion of a large number of past situation into a general concept of what constitutes a reasonable pressure configuration under the given con ditions. This is largely a subjective process as indicated by the frequent divergences of opinio among equally competent forecasters. Numerous attempts have been made to introduce objectiv methods based on some theoretical grounds, but it appears that none of these is as effective as th subjective methods practiced by an experienced forecaster. In view of this it is not surprising tha many forecasters with little or no formal training are able to compete on equal, or better, terms wit the products of our best university training.
>
> Such comparisons as have been made indicate that there has been no significant increase i the accuracy of short-range forecasts in the past 30 or 40 years. Such comparisons must be mad on forecasts of temperature or precipitation since airway and upper-level forecasting are modern developments. However, I believe that it is mere quibbling to argue the point as to whether fore casting accuracy has improved. The number of elements forecast, the frequency of forecasts, and the types of forecasts have all increased greatly, but it is clear that there has been no corresponding increase in accuracy.
>
> There appears to be no immediate prospect of an objective method of forecasting based entirely on sound physical principles. If we are to attain this goal in a reasonable time, it is essential that the subjective nature of present methods be kept clearly in mind. The vast store of empirical data which we have accumulated is essential for the guidance of the scientific attack but the unaided empirical or synoptic method has approached or reached the point of diminishing returns.
>
> Since we cannot foresee an early solution to the forecasting problem on a purely physical basis, it is reasonable and proper for us to attempt to improve present methods. It must always be remembered that such efforts are in the nature of palliatives rather than cures.

The reference to the absence of significant increase in accuracy in the last 30 or 40 years presumably refers to the Weather Bureau checks. A system of checking of 24 to 36-hour forecasts fixed long ago is unlikely to do justice to the actual progress made, which is mainly in providing detailed and fairly accurate forecasts for the first 12 hours. The increase of detail is in itself an increase of accuracy, in one sense of that term. Checks of rainfall and temperature forecasts are not altogether satisfactory. Qualitative forecasts of rainfall are often difficult to check owing to the vague phraseology which is sometimes unavoidable, and is certainly preferable to a completely wrong forecast. Quantitative forecasts of rainfall show poor results owing to the irregularities due to instability and other factors. Quantitative forecasts of temperature are better, but it is beyond doubt that the most important requirement for a high standard is long experience of forecasting temperature at the place or places which are checked, and not an expert knowledge of the latest upper-air techniques. In the British Isles we have benefited from an increase of observations to westward, and it would be hopeless even to attempt to assess how much of the improvement in general forecasting

can be attributed to factors other than increased information. Better training and the much larger number of high-grade forecasters now available have led to an advance, but though this is very important it cannot go beyond a certain point.

10. The modern period

The distinguishing feature of the modern period, which is still in its infancy, is the attempt to apply three-dimensional reasoning, based on dynamical principles, to the general development problem. Its origins extend far back, but till recently the observational material was inadequate for anything but rare and sketchy practical applications, especially in the British Isles, where moderately adequate information has only been available since late in 1947. The most immediate effect of the new information is to improve upper-wind forecasts and to extend the range of height and the time intervals for which they are reasonably good. It might appear to the man in the street that if good forecasts can be made of winds across the Atlantic it should be possible to tell without fail whether tomorrow will be wet or fine, but whereas the wind forecasts depend on first-order terms in the equations of horizontal motion, the rainfall problem also involves second-order terms. As pointed out by Sutcliffe and Forsdyke (1950) the increased information has not been an unmitigated blessing, since ' we are rather suddenly provided with more data than we can easily digest, represented on numerous upper-air charts and aerological diagrams.' The quotation from Houghton in the previous section is relevant to this, since in much of the U.S.A. they had upper-air information to westward for some years before 1946. The atmosphere is in a continuous state of flux and the forecaster has only a limited time at his disposal. If the upper-air charts are to be of any real use for 24 to 30-hour forecasting they must be adequately dealt with in a few hours, since all charts soon become out-of-date. If a forecaster spends too much of his time in examining what he cannot digest he may damage rather than improve his forecast. The upper charts have to be dealt with by a separate roster of forecasters, which make forecasting more of a group activity and less individual, but someone has to make the final decision, so that there is still a subjective element. The amount of subjectivity could only be tested by having forecasters working independently on the same data, but it is generally recognized to be appreciable though on the average a good deal less than the difference between forecast and actual events. When the atmosphere behaves in an abnormal way it dupes all forecasters, and though some may be more surprised than others, nobody can really cope with the situation.

The existence of a roster of upper-air forecasters does not dispense with the necessity for simplication for the purpose of general 24-hour forecasting. The position is analogous to that already mentioned in connection with frontal analysis, with which it is closely related. The complex details, like the very elaborate analyses, have only a very short-term validity. The simplications advocated by Sutcliffe (1947) and by Sutcliffe and Forsdyke (1950) should be judged in the light of practical necessities.

For some time past a great deal of research in several countries has been devoted to the main problem of development, of which the most fundamental feature is the vertical motion and the associated field of divergence, which reverses its sign at the level of maximum vertical motion, generally at about 650 or 600 mb. The barometric tendency at sea level represents the integral of the divergence of the whole overlying column of air and has the nature of a small residue, normally the difference between

much larger divergences of opposite sign in the upper and lower troposphere, which are themselves second-order effects. Thus the tendency is a third-order product of the air motion, and this makes a direct attack on it almost hopeless. Since the divergence cannot be integrated nearly so well as is done automatically by the tendency field itself, there is no way of adding together the changes in a succession of atmospheric layers, and they are never independent. The key to the whole problem is the vertical motion. In some situations this can be roughly measured by its effect on temperature and humidity, but in the most important cases this is impossible as ascending saturated air is generally in a near-neutral condition. Observations of the rate of rainfall on a sufficient scale might be of use, but over the sea there are observational difficulties. In America a very big effort is being devoted to these problems, and an attempt is being made to use electronic machines in the effort to solve them. This experiment will be watched with interest, but at present the only realistic hope is that the machine may help in forecasting some large-scale developments, leaving the detail to be filled in by other means. If the attempt succeeds it will be a notable advance, more especially for the ' further outlook.' It is doubtful, however, to what extent the large so-called waves (troughs and wedges) at 500 mb, which are the present object of attack, are really independent of ordinary depressions and anticyclones, which can be regarded as atmospheric organisms with a phase of growth and then of slow decay.

On the whole the prospects of computing the future weather, with or without electronic machines, look remote at present. Nevertheless the introduction of quantitative elements, such as have been brought into the study of vorticity and divergence by Sawyer, Sheppard, and others, do represent a real advance, since it leads to a more precise understanding of the atmospheric processes. This must have some effect on forecasting, which is based on what Sutcliffe has called ' rational empiricism,' that is to say an empiricism based on a partial understanding of the physical and dynamical process underlying the observed developments. All existing techniques are empirical in this sense, and are broadly similar in the temperate zones, though there are differences in detail between various services. The 500 mb charts are usually considered the best supplementary ones for general use as they are representative of the upper troposphere and are normally above the level of maximum vertical velocity. The 300 mb chart is very similar but has steeper general gradients, which makes the detail more difficult to pick up, and it sometimes cuts the stratosphere. Both the absolute contours giving the flow patterns on the isobaric surfaces, and the thicknesses, which depend on the mean temperature of the column of air between two isobaric surfaces, are now in widespread use, and in the upper troposphere they have a strong resemblance. Dr. Sutcliffe has stressed the importance of thicknesses on both thermodynamical and on dynamical grounds, especially the 1,000-500 mb thicknesses. The fact that the divergence normally reverses sign below the 500 mb surface means that the divergence of the thermal wind is dominant aloft and it is this, and only this, which is directly related to the sea-level pressure tendency, in the sense that withdrawal of air corresponds with falling pressure. Development of circulation, including not only deepening lows and building highs but much of the movement of pressure systems (that part which is not a pure transport in a general stream) necessarily implies vertical movement. The development patterns of Sutcliffe are specially relevant to deepening and building and also to dissipation, though friction also affects this process, which is on the whole less important than development. Models of this type cannot provide solutions applicable to all cases, but they are a useful aid to empirical forecasting. In a forecast of

development in the next 6 to 12 hours it is obvious that great weight must be attached to existing trends, so that the problem resolves itself into that of understanding the existing trends and then estimating how much they are likely to be modified during a 24-hour period. Forecasts of changes of thickness are of course made use of as far as possible, but these are partly based on the prebaratic, and the time available for a process of mutual adjustment is small and often nil. Whenever the situation is changing quickly forecasting is a race against the clock. The prognostic value of thermal patterns, and of all empirical applications of upper-air charts, is greatest when a small system moves into a different thermal field which will alter its speed and direction and may favour either development or dissipation. Larger systems are much more difficult to deal with, but there are cases when even a fairly large system moving into a new field is bound to be affected, though accurate computations are impossible. The reaction of the system of air movement on the thermal field is a serious difficulty. There is a well-known self-generating process of depressions and anticyclones, which create their own developmental thermal pattern, and the speed of the system also comes into this problem, and these processes limit predictability. When the time interval exceeds 24 hours, the relative value of three-dimensional reasoning is greater, though the absolute errors of forecast charts obviously increase with the time interval.

Among other aspects of modern forecasting, only a few of the most important can be briefly referred to. A good prebaratic chart does not ensure a good forecast though it goes a long way towards it. The forecast of precipitation may be in error even if the main features of the prebaratic are right, but in such cases the prebaratic normally contains errors of detail, particularly in the distribution of cyclonic vorticity. This is of course itself largely the result of vertical motion, and until the circle can be broken into there is little chance of a major advance. Convergence and its related vorticity distribution is also very important for the development of thunderstorms and rain of thundery origin, which includes a high proportion of summer rainfall over most of England. Vertical soundings plotted on tephigrams lead to good though not infallible forecasts for a short period, but in a 24-hour period the changes of vertical stability are usually appreciable and are very difficult to predict. Horizontal advection does not provide an adequate basis for a forecast, and J. M. Craddock (1951) has shown that the advective temperature change is heavily counteracted by other factors and that the resultant change is a residual having a correlation of 0·6 with the advective component. The relation between thunderstorms and convergence is very obvious, and divergence quickly stabilises almost any air mass, so that for forecasting thunderstorms for the next day a good prebaratic is a most essential tool. Thus forecasting thunder for the next day is still rather similar in principle to what it was early in the century, when fairly good results were obtained, even though a fair measure of understanding has replaced almost complete empiricism.

The location of existing thunderstorms by means of the ' sferic ' apparatus has its main application to short-period forecasting, but in some situations, particularly when there are outbreaks over the Bay of Biscay or off Portugal, it is valuable for forecasting for the following day. In summer and autumn successive groups of thunderstorms often travel in the currents at medium levels in a baroclinic zone. Each group has a phase of maximum activity of a few hours and is then apt to degenerate into rain without thunder. As with other data used in forecasting, they must be studied in conjunction with other information, and the successive phases must be understood, including such matters as the early-morning maximum over the sea areas.

Fog forecasting also depends on prebaratics to the extent that except for very short time intervals an accurate forecast of the geostrophic wind is the most difficult part of it. If one has a good wind forecast the problem can be isolated to a greater extent than most of the major forecast problems, but a good deal of empiricism is still necessary in fog forecasting, and the physical problem is difficult. If one goes a little higher up and considers low clouds, including stratus, the problem is less isolated and the general vertical motion often comes into it.

11. Conclusion

The main conclusion from this survey is that the large advance made in observations and communications during the half-century has led to the development of fairly accurate detailed forecasts for the first 12 hours, extending up to high levels, and to some improvement in the standard of 24 to 30-hour general forecasts. Concurrently there has been a great increase in our understanding of the physical and dynamical atmospheric processes, which has reacted on forecasting and made it more interesting, though there is no means of estimating how much of the forecasting advance can be attributed to it. A good deal of the improvement can be attained by a purely empirical *ad hoc* application of the new information. Even if the whole improvement could be explained in this way, forecasting would be a branch of applied science, since the observations are obtained and transmitted by scientific means. Actually there is no reason to doubt that increased scientific understanding has had some positive result, though it is disappointingly small, especially from the point of view of the scientific forecaster. Any future advance must depend on scientific advance in synoptic meteorology itself, as the possibilities of improvement merely from new data now look small in our area.

Even a limited advance may make a big difference to the real value of the forecasts, as compared with those attainable by statistical methods. The recent great increase in the public interest in the forecasts may be partly due to this, though other factors are helping to produce this. There are substantial and increasing economic applications over a wide field, which is very satisfactory in itself, though it also throws into greater relief the limitations of present-day forecasting.

We may conclude by a quotation from the last page of Shaw's *Forecasting Weather* :

> It may be that in the end the caprices of weather will after all disappoint us, and tomorrow's weather will never be forecasted with sufficient accuracy for all practical purposes. Even in that case the attempt will not have been in vain.

In one sense the doubt has been removed, in that a forecast service is essential in a modern community. But the larger question remains as to whether the forecasts can be made really satisfactory both to the physicists who make them and to the public who use them. After another few decades it should be possible to give a more definite answer to this question.

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The spontaneous crystallization of supercooled water

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Summary

It is shown theoretically that the rate of nucleation of the ice phase in supercooled water changes very rapidly with temperature in the neighbourhood of − 40°C. Thus, in chilling a supercooled water cloud below this temperature one would expect to observe a rapid increase in the number of ice crystals which appear as the temperature falls. This is verified by observations made in a cold chamber. The different critical temperatures for the spontaneous appearance of ice crystals quoted by Schaefer (− 39°) and the Oxford workers (− 41°C) probably arise as the result of differences in the experimental conditions. It is tentatively suggested that the parts of mother-of-pearl clouds showing iridescence at temperatures around − 80°C consist of spheres of 'glassy' ice, which may eventually crystallize spontaneously to form ice crystals towards the leeward edges of the cloud.

Introduction

It has been reported by both Cwilong (1947) and Fournier D'Albe (1949) that small droplets of water and dilute aqueous solutions suspended in air freed from foreign nuclei, can be super-cooled to $-41° \pm 0{\cdot}2$°C, below which an ice cloud appeared in their expansion chambers. Schaefer (1948), however, quotes a critical temperature of − 38·9°C at which ice crystals are produced in a supercooled cloud by passing a cold body through it. He reports that a pellet of mercury (melting point − 38·9°C) leaves ice crystals in its wake as long as it remains solid, but, at the instant of melting, this property disappears. He interprets this temperature as the critical temperature at which ice crystals arise spontaneously from the *vapour*. Very recently, Lafargue (1950) has found that droplets of pure water and various salt solutions always freeze at $-40{\cdot}5° \pm 1{\cdot}5$°C if their diameters are less than about 20 μ. On the other hand, there is evidence from mother-of-pearl clouds (Störmer 1948) and from reports on aircraft icing (Aanensen 1948), that water can exist in the supercooled state at lower temperatures, perhaps down to − 80°C.

It seems unlikely that spontaneous crystallization, an essentially statistical phenomenon, will be associated with a sharp transition temperature. In this paper it is shown that the results quoted above are not incompatible, but can be interpreted in terms of the time required for spontaneous nucleation of the ice phase in supercooled water. The theory is also supported by some additional laboratory observations.

2. Theory

In the absence of foreign nuclei, aggregates of the ice phase will be formed by chance associations of molecules of the supercooled water ; such molecular complexes continually arise and disappear as a result of thermal fluctuations. The lower the temperature the greater is their size and frequency of formation until, eventually, they attain a critical size above which they survive and continue to grow, forming nuclei for the development of the ice phase.

An approximate expression for the rate of formation of nuclei in a condensed phase has recently been derived by Turnbull and Fisher (1949), *viz.*,

$$J \simeq \frac{nkT}{h} \exp\left[-(U + W_c)/kT\right] \quad . \quad . \quad . \quad . \quad (1)$$

where J is the rate of production of nuclei cm^{-3} sec^{-1}, n the number of molecules per cm^3 of the condensed phase, k Boltzmann's constant, h Planck's constant, T the absolute temperature, U the activation energy for self-diffusion of a molecule in the liquid and W_c the work of nucleus formation.

Now, $$W_c = \tfrac{1}{3}\sigma S = \tfrac{1}{3}\sigma\omega r_c^2 \text{ (Frenkel 1946)} \quad . \quad . \quad . \quad (2)$$

where σ is the specific surface energy of the crystal-liquid interface, S the surface area of the nucleus, r_c the radius of the sphere inscribed in a nucleus of critical size and ω a shape factor expressing the surface/volume ratio of the nucleus. As ice crystallizes in the hexagonal form we shall take the nucleus to be an isometric hexagonal prism for which $\omega \simeq 23$. The radius r_c is given by Thomson's equation :

$$r_c = \frac{2\sigma}{\rho L} \frac{T_0}{T_0 - T} \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

where ρ is the crystal density, L the latent heat of fusion, and T_0 ($= 273°K$) the thermodynamic freezing point.

On substituting numerical values, Eq. (1) can be written in the form

$$\log J = 32{\cdot}84 + \log T - \frac{U}{2{\cdot}303\,kT} - \frac{760\,\sigma^3}{(T_0 - T)^2\,T} \cdot \quad . \quad (4)$$

U is related to the viscosity η of the liquid by the equation $\eta = Ae^{U/kT}$ (Andrade 1934). The viscosity of the supercooled water has been measured down to $-10°C$ only (White and Twining 1913), the value of U over the range $0°C$ to $-10°C$ being $3{\cdot}3 \times 10^{-13}$ erg. We shall assume that the value of U at $-40°C$ is not greatly different from this. The most difficult parameter in Eq. (4) to evaluate is σ, which may be taken as the difference between the surface tensions of ice and water at the same temperature. The surface tension of ice has not been calculated as the precise nature of the force field existing between the molecular dipoles is not known. One may make a rough calculation by considering the work necessary to separate two surfaces parallel to say, the (0001) plane, in which the forces between neighbouring molecules only are considered. This would involve rupture of two hydrogen bonds per unit cell, the bonds being normal to the (0001) face. Pauling (1940) calculates the strength of the hydrogen bond of water molecules to be 4,500 cal/mol, the corresponding value for ice at $-40°C$ being about 5,100 cal/mol. The area occupied per bond being $3^{\frac{1}{2}}\,a^2/2$ ($a = 4{\cdot}52 \times 10^{-8}$ cm), the surface tension of ice is calculated to be 102 erg cm^{-2}. Taking the surface tension of supercooled water at $-40°C$ to be 80 erg cm^{-2}, this gives 22 erg cm^{-2} for the interfacial surface energy σ. In view of the uncertainty in the calculated value of σ, and the fact that the values of the nucleation rate predicted by Eq. (4) are very sensitive to the adopted value of σ, it is not worth while to calculate absolute values of J as a function of temperature. It may be shown, however, that the nucleation rate decreases very rapidly with temperature around $-40°C$, and that the value of d/dT ($\log J$) is not very sensitive to the value chosen for σ. Differentiating Eq. (4) with respect to T,

$$\frac{d}{dT}(\log J) = \frac{1}{T} + \frac{U}{2{\cdot}303\,kT^2} - \frac{760\,\sigma^3(3T - T_0)}{(T_0 - T)^3\,T^2} \quad . \quad . \quad (5)$$

Putting $T = 233°K$, the other parameters having the values quoted above, gives $d/dT(\log J) = -0{\cdot}93$ showing that the nucleation rate increases about nine-fold for each degree fall in temperature around $-40°C$. An error of ± 5 per cent in the value of σ would cause this factor to vary between 12 and 7 only.

3. Observations in a cold box

The trend suggested by the above theory was tested by observing very carefully the variation in concentration of ice crystals produced in a supercooled cloud by drawing through it a cold body maintained at different temperatures in the neighbourhood of $-40°C$. The apparatus consisted of a thermostatically controlled low-temperature refrigerator of about 2 ft^3 capacity, the inner walls of which were blackened to provide dark-background illumination and painted with glycerol to prevent frost formation. A supercooled cloud was formed by wafting a damp cloth in the chamber, the temperature of which was kept constant at about $-10°C$, when only a very occasional natural ice particle was observed. The cold test body consisted of just the end face of a copper rod (diameter 1 cm), the other end of which dipped into a flask of liquid oxygen, the rest of its surface being well lagged. The temperature of the test surface was controlled by a heating coil and measured by a thermocouple, the reference junction of which was coated with a very thin film of plastic and placed in melting mercury. The numbers of ice crystals produced by the rod when cooled to successively lower temperatures were observed in the beam of a high-pressure mercury arc, the cloud being renewed for each observation.

Only a very few crystals were observed in the path of the test body while its temperature was $-39°C$ but the number rapidly increased as its temperature was reduced ; below $-41°C$ the cloud was quickly transformed entirely into ice. At the higher temperatures there was a few seconds' delay between the passage of the rod and the appearance of the crystals, but crystals appeared almost instantaneously at $-41°C$ and below.

4. Discussion of experimental results

It does not seem likely that when a supercooled cloud is chilled locally below $-39°C$ ice crystals arise spontaneously direct from the *vapour* as envisaged by Schaefer. If a body at $-40°C$ were suddenly introduced into a water cloud initially at say $-10°C$ a very high supersaturation would be produced momentarily in the immediate neighbourhood of the body – high enough to produce larger numbers of minute *droplets* by spontaneous condensation (Mason 1951). If the minimum temperature persists for a time sufficient to allow these droplets to crystallize spontaneously, they will survive and continue to grow to visible size, since even at the original temperature of the cloud their environment will be supersaturated with respect to ice. If however they cannot freeze the droplets will evaporate before becoming visible. That new aggregates arise out of the vapour and that the appearance of ice crystals is not merely due to freezing of pre-existing droplets is indicated by the fact that, if the test body is sufficiently cold, the number of ice crystals appearing greatly exceeds the original number of water droplets. If the ice crystals were arising spontaneously from the

apour it would be difficult to explain why they should arise in observable quantities t very nearly the same temperature as that at which small water droplets are observed to eeze ; it seems more likely that the same physical process is involved in each case. In ny case, theory indicates that ice crystals would arise from the vapour at a *lower* tempera- ure than from supercooled droplets, whereas Schaefer's critical temperature is the igher. Krastanow (1940) calculated that the direct condensation of pure vapour nto ice would not occur until a threshold temperature of $-70°C$ was reached ; how- ver, this calculated result is so sensitive to small changes in the values assumed for ome of the parameters that no confidence can be attached to it.

If ice crystals are to appear in a suddenly chilled cloud, the minimum temperature ttained must persist for a period greater than the corresponding nucleation time of a roplet. The chance of observing an ice crystal in a given time interval will depend n the prevailing temperature, the size and the number of droplets in the field of view.

In the cold-box experiments described in Section 3, the rod of diameter 1 cm was rawn through the cloud at a speed of about 1 cm/sec, so the air in immediate contact vith the rod would be cooled to its surface temperature for a period of about 1 second. An auxilliary experiment, in which the cloudy air was allowed to flow past a stationary phere with surface temperature $\simeq -40°C$, indicated that ice crystals were forming n a sheath of air 2/3 mm thick. Hence, it is estimated that in drawing the cold rod hrough the cloud about 20 cm^3 of air were cooled to very nearly the surface tempera- ure of the rod.

The small number of ice crystals observed at $-39°C$ and just below probably esulted from the freezing of some of the cloud droplets which were present in con- centrations of a few hundred per cm^3 with radii between 5 and 10 μ. The much maller droplets produced by spontaneous condensation appeared as a dense but ransient fog ; presumably they had insufficient time to freeze before they were warmed bove $-39°C$ and thereafter evaporated. The rapid increase in the number of crystals ppearing at lower temperatures was in accord with the nucleation rate increasing bout ten-fold for each degree fall in temperature, while at temperatures appreciably below $-41°C$, the number of ice crystals formed far exceeded the original number of water droplets, showing that the droplets arising spontaneously from the vapour were now being crystallized.

Cwilong and Fournier D'Albe were observing the condensation products formed by a rapid expansion of saturated air in a cloud chamber, with particular reference to the transition from water droplets to ice crystals as a function of the temperature prevailing at the end of the expansion. They observed no ice crystals until this tempera- ture fell below $-41°C$. There are several contributory factors which may explain this. First, the sensitive time of the small chambers ($\simeq 0{\cdot}1$ sec in Fournier D'Albe's apparatus) was considerably less than the one second for which the air was cooled in the cold box. Secondly, they were observing smaller droplets ($r < 5\ \mu$) ; hence, the time required for self-nucleation (which is inversely proportional to the volume of the droplet), would be perhaps ten times greater than for the larger droplets in the cold box. Thirdly, Fournier D'Albe was observing only a very small volume, about $0{\cdot}2$ cm^3. Also, in the expansion-chamber experiments, rapidly growing droplets were observed whose surface temperatures may have been about $0{\cdot}5°C$ higher than that of the air. These factors taken together might very well explain why ice crystals appeared at a temperature 2°C higher in the cold-box experiments.

It appears then, that the values which different authors have quoted for the appear-

ance of ice crystals are all consistent with the theory that there is no clear-cut transition temperature, but a rapid change of nucleation rate with temperature around — 40°C and that the apparent disagreement is due to the different conditions of observation

5. The physical state of mother-of-pearl clouds

Although it appears that small water droplets will not exist in the supercooled state for more than a fraction of a second at temperatures around — 41°C, the coloured interference bands formed by mother-of-pearl clouds suggest that the windward edges of the clouds consist of water droplets not exceeding 2·5 μ in diameter (see Störmer 1948, Weickmann 1947), although the temperature may be as low as — 80°C. Some clouds show a decrease of iridescence from the windward edge, which is thought to be due to a transition from the water to the ice phase within the cloud. Weickmann suggests that an explanation of these phenomena might be found in the work of Rau (1944) who claimed to have supercooled liquid water to — 72°C. However, these results are strongly suspect, since both Cwilong, and Brewer and Palmer (1951), see also Mason and Ludlam (1951), failed to reproduce Rau's results except when their samples were deliberately contaminated with alcohol.

Now, if U and σ are regarded as independent of temperature, Eq. (4) predicts that the rate of nucleation will increase with decreasing temperature until the temperature, given by setting the right-hand side of Eq. (5) equal to zero is reached, and thereafter will fall. We do not know how U and σ for water vary with temperature in the supercooled region, but experiments with other supercooled liquids show U to increase at lower temperatures (*e.g.*, Dodd and Mi 1949), while the value of σ will probably change only slightly. We also know that a ' glassy ' ice forms at about — 80°C on the thimbles of frost-point hygrometers, indicating that at about this temperature ice undergoes a structural change with a very marked increase in viscosity and hence in the activation energy U. This would tend to slow down the rate of self-nucleation and also the velocity of linear crystallization, so that the glassy ice would change to the crystalline form only very slowly. This probably explains why the surface of a frost-point hygrometer which cannot form a frost deposit initially at these low temperatures becomes activated after a long period, see Dobson (1949). It is then suggested that, in the neighbourhood of — 80°C, the region of the mother-of-pearl cloud showing iridescence is composed of small spheres of ' glassy ice ' which may eventually crystallize spontaneously to form ice crystals at the leeward edge of the cloud, where the iridescence disappears.

6. The icing of aircraft at temperatures below — 41°C

Aanensen (1948) describes a Mosquito aircraft producing propeller trails when climbing from 400 mb (— 44°C) to 300 mb (— 58°C), clear ice being formed on the wings immediately behind the propellers. There was no cloud at that height and no ice formed on the aircraft except when immersed in the condensation trails. The air (whose humidity was about 90 per cent) was no doubt cooled below the dew point in the immediate vicinity of the propellers. It is suggested that the time taken for the droplets to move from the propellers to the leading edge of the wing ($<$ ·01 sec) was less than the nucleation time of the droplets at — 44°C. This time cannot be calculated from Eq. (4) with any confidence for the reasons already given and because the size

the droplets is unknown. If, however, a droplet of $r = 10\ \mu$ takes 1 sec to freeze the cold box at $-40°C$, and the nucleation rate changes with temperature as calculated in Section 2, a droplet of $r = 2\ \mu$ would take about 0·1 sec at $-44°C$. Although the aircraft continued to form condensation trails at lower temperatures there is no real evidence that *ice* was forming continuously; indeed, as ice was forming so slowly, the requisite observation would be very difficult to make.

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Effect of turbulence and condensation on drop-size distribution in cloud

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SUMMARY

Equations are developed to represent the rate of change of the number of drops of a given size in a cloud owing to condensation and to coalescence respectively. Numerical computation from these equations shows that condensation has a much greater effect than coalescence on drop-size distribution in clouds with small average drop size, with small liquid-water content or with a large degree of supersaturation.

With the assumption that drop-size distribution in a cloud is governed by rate of condensation on the drops in the cloud and rate of turbulent diffusion of the drops out of the cloud it is then shown that the fractional volume of water comprised by drops smaller than a specified size is expressible in terms of an Incomplete Gamma function. In this Incomplete Gamma function the argument is 1·5 and the upper limit to the integral varies as the square of the drop size and is also proportional to a quantity which depends upon the degree of turbulence and upon parameters connected with the rate of condensation. It is shown that this theoretical formula approximates closely to a simpler formula already established empirically and that reasonable values for the coefficient of turbulence and the condensation parameters lead to a mean drop size which agrees closely with measured values.

1. INTRODUCTION

In an earlier paper (Best 1951a) it was shown that the average size of cloud particles is consistent with the assumption that this size is determined by the condensation processes operating for the average time during which an individual cloud droplet remains in the cloud before being ejected under the influence of turbulent diffusion. In a second paper (Best 1951b) it was shown that measured drop-size distributions in cloud conform with reasonably close approximation to the formula

$$1 - F = \exp\left[-(x/b)^n\right] \qquad (1)$$

In the present paper we shall examine the magnitude of the effect of coalescence on size distribution of cloud drops and shall compare it with the magnitude of the effect of condensation on size distribution. When considering coalescence we shall assume that two drops coalesce when they touch though possibly this is controversial. We shall also assume a collection efficiency of unity for a cloud drop falling through a group of smaller drops. This will lead to an overestimation of the effects of coalescence. Nevertheless we shall see that frequently the effect of coalescence is small compared with the effect of condensation. It is then a natural sequel to the earlier work to consider whether the turbulent diffusion and the condensation processes can explain the form of the drop-size distribution formula found empirically.

2. THE EQUATIONS

Consider a cloud layer which is uniform in the horizontal direction. At any point in the cloud denoted by the vertical co-ordinate z let the number of drops per unit volume and with radius between r and $r + \Delta r$ be $f(r, z, t)\,\Delta r$ or, more shortly, $f\,\Delta r$.

If we neglect sedimentation the factors which govern f are :

(a) All drops are growing by condensation so that drops within the range r to $r + \Delta r$ tend to grow out of this range and smaller drops tend to grow into it.
(b) All drops are growing by absorbing smaller drops so that drops within the range tend to grow out of it and smaller drops tend to grow into it.
(c) Some drops within the range disappear because they have been absorbed by larger drops.
(d) f varies with distance from the (horizontal) boundary surface of the cloud. Accordingly turbulent diffusion is continuously transporting drops of all sizes from inside the cloud to the boundary and thence into the clear air where they evaporate.

The condensation processes are described by the equation (see e.g., Fuchs 1934)

$$r\frac{\partial r}{\partial t} = G \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where

$$G = \frac{MDsp_\theta}{R\theta} \quad . \quad . \quad . \quad . \quad (3)$$

r = drop radius
M = molecular weight of water (18)
D = coefficient of diffusion of water vapour in air ($cm^2\ sec^{-1}$)
R = gas constant per mol ($= 8{\cdot}314 \times 10^7$ erg $deg^{-1}\ mol^{-1}$)
θ = absolute temperature
sp_θ = vapour-pressure excess of water in atmosphere over drop (dynes cm^{-2})
p_θ = saturation-vapour pressure of water at temperature θ (dynes cm^{-2})

(Except for very small drops 100 s is practically the excess of relative humidity over 100 per cent).

Thus G is independent of r. We are primarily concerned here with conditions in a layer-type cloud. If we consider a cloud with base at 950 mb, temperature 273°A and top at 850 mb, temperature 268°A, the cloud thickness will be about 2,900 ft and the value of $D\,p_\theta/\theta$ at base and top will be 517 and 390 CGS units respectively. There are no measurements available to indicate the variation of s with height but since the supersaturation arises from the lifting of air it seems likely that s will be somewhat greater near the top of the cloud than near the base, though since condensation is proportional to s the difference is unlikely to be large. With these considerations in mind we shall assume that G is independent of z.

If we denote the time rate of variation of f owing to condensation by f_n' then

$$f_n' = -\frac{\partial}{\partial r}\left(f\frac{\partial r}{\partial t}\right) = -\frac{\partial}{\partial r}\left(f\frac{G}{r}\right) \quad . \quad . \quad . \quad . \quad (4)$$

The influence of coalescence is represented by the two factors (b) and (c) above. The rate of variation of f arising from factor (b) can be computed as follows. Let the terminal velocity of a drop of radius r be $u(r)$. In time Δt the swept volume of contact for drops of radius r and of radius $x\ (< r)$ is $\pi\,(r + x)^2\,[u(r) - u(x)]\,\Delta t$. Since the falling drop is assumed to collect all smaller drops lying in its path we have

$$\frac{\partial}{\partial t}\left(\frac{4\pi r^3}{3}\right)\Delta t = \int_0^r f(x, z, t)\frac{4\pi x^3}{3}\,\pi\,(r + x)^2\,[u(r) - u(x)]\,dx\,\Delta t$$

or

$$\frac{\partial r}{\partial t} = \frac{\pi}{3r^2}\int_0^r x^3 f(x, z, t)\,(r + x)^2\,[u(r) - u(x)]\,dx \quad . \quad . \quad (5)$$

Eq. (5) represents the rate of growth of an individual droplet owing to coalescenc during the period before it is in turn absorbed by a larger drop. The time rate c variation of f owing to absorption of smaller drops is then given by

$$f_a' = -\frac{\partial}{\partial r}\left(f\frac{\partial r}{\partial t}\right) \quad . \quad . \quad . \quad . \quad (6$$

where $\partial r/\partial t$ is given by Eq. (5).

Turning now to the rate of disappearance of drops owing to absorption by large drops we easily get

$$f_d' = -\int_r^\infty f(x, z, t)\, f(r, z, t)\, \pi (x + r)^2 \left[u(x) - u(r)\right] dx \quad . \quad (7$$

where the suffix d denotes the time variation of f owing to absorption by larger drops.

For the present we shall not try to determine f but shall compute $(f_a' + f_d')$, the effect of coalescence, and compare it with f_n', the effect of condensation. To do this we shall assume that the drop-size distribution is defined by

$$1 - F = \exp\left[-(r/b)^n\right] \quad . \quad . \quad . \quad (8)$$

where F is the fraction of liquid water in the air comprised by drops with radius less than r and b and n are constants with approximate values 10 μ and 3·25 respectively. Eq. (8) has been justified on empirical grounds elsewhere (Best 1951b). From Eq. (8) it easily follows that

$$f = \frac{3Wnr^{n-4}}{4\pi b^n} \exp\left[-(r/b)^n\right] \quad . \quad . \quad . \quad (9)$$

where W is the concentration of liquid water in the air (cm^3/cm^3). We shall also assume the formula obtained from Stokes's law for the terminal velocity of a drop. Neglecting air density in comparison with the density of water this gives

$$u(r) = \frac{2gr^2}{9\mu} \quad . \quad . \quad . \quad . \quad . \quad (10)$$

where μ is the coefficient of viscosity of air.

3. Numerical computation

Substituting from Eqs. (9) and (10) in Eqs. (5) and (6) we obtain after a little algebraic manipulation

$$f_a' = -f\frac{Wgb}{18k^2\mu} P \quad . \quad . \quad . \quad . \quad (11)$$

where

$$k = r/b$$

$$P = A_0\left[1 - \exp(-k^n)\right] + A_1\,\Gamma(1 + 1/n)\, I\left[k^n(1 + 1/n)^{-1/2}, 1/n\right]$$
$$+ A_3\,\Gamma(1 + 3/n)\, I\left[k^n(1 + 3/n)^{-1/2}, 3/n\right]$$
$$+ A_4\,\Gamma(1 + 4/n)\, I\left[k^n(1 + 4/n)^{-1/2}, 4/n\right]$$

$$I(u, p) = \int_0^{u(p+1)^{1/2}} \frac{e^{-v} v^p}{\Gamma(p+1)}\, dv$$

$$A_0 = k^3 (n - 2 - nk^n)$$

$$A_1 = 2k^2 (n - 3 - nk^n)$$

$$A_3 = -2 (n - 5 - nk^n)$$

$$A_4 = -\frac{1}{k} (n - 6 - nk^n)$$

The particular form chosen for the function P was selected in order to facilitate the use of Pearson's tables of the Incomplete Gamma function).

From Eqs. (4) and (9) we get

$$f_n' = f \frac{A_3 G}{2b^2 k^2} \quad . \quad . \quad . \quad . \quad . \quad (12)$$

Similarly from Eqs. (7) and (9) we obtain

$$f_d' = -f \frac{Wgb}{6\mu} Z \quad . \quad . \quad . \quad . \quad . \quad (13)$$

where

$$Z = \int_{k^n}^{\infty} e^{-y} (y^{1/n} + 2k - 2k^3 y^{-2/n} - k^4 y^{-3/n}) \, dy$$

The numerical evaluation of Z depends upon the value of n. It is easily verified that

$$n = 1, \quad Z = \frac{e^{-k}}{2} (4k - 5k^2 + k^3) + \Gamma(2) [1 - I(k/2^{1/2}, 1)] - \frac{k^3}{2} \mathrm{Ei}(-k) (4 - k)$$

$$n = 2, \quad Z = 2k e^{-k^2} (1 - k^2 - 2k^4) + \Gamma(3/2) (1 + 4k^4) [1 - I(k^2 2^{1/2}/3^{1/2}, 1/2)] + 2k^3 \mathrm{Ei}(-k^2)$$

$$n = 3, \quad Z = 2k e^{-k^3} (1 + 3k^3) + \Gamma(4/3) (1 - 6k^3) [1 - I(k^3 3^{1/2}/2, 1/3)] + k^4 \mathrm{Ei}(-k^3)$$

$$n = 4, \quad Z = 2k e^{-k^4} (1 + 4k^4) + \Gamma(5/4) (1 - 4k^4) [1 - I(2k^4/5^{1/2}, 1/4)] - 4k^3 \Gamma(3/2) [1 - I(k^4 2^{1/2}/3^{1/2}, 1/2)]$$

where $\mathrm{Ei}(-x)$ is the exponential integral $\int_{\infty}^{x} (e^{-y}/y) \, dy$. It is to be noted that the radius of the droplet is now given by k, the unit being the scale radius b.

4. Numerical results for specified conditions

From Eqs. 11, 12 and 13 we see that

$$\frac{f_n'}{f} = \frac{A_3 G}{2b^2 k^2} \quad . \quad . \quad . \quad . \quad . \quad (14)$$

$$\frac{f_a' + f_d'}{f} = -\frac{Wgb}{6\mu} \left[\frac{P}{3k^2} + Z \right] \quad . \quad . \quad . \quad (15)$$

The fractional rate of increase of f owing to condensation (Eq. 14) depends upon the values of the constants representing the physical characteristics of air and water, upon the concentration of liquid water W, upon the parameters b and n in the assumed drop-size distribution and upon the radius k $(= r/b)$ of the drop. We are primarily interested here in the ratio of the condensation term to the coalescence term

$$\frac{f_n'}{f_a' + f_d'} = -\frac{\mu G}{Wb^3} \frac{3A_3}{gk^2\left(\frac{P}{3k^2} + Z\right)} \quad . \quad . \quad . \quad . \quad . \quad (16$$

the significant question being whether this ratio differs substantially from unity. Let us select values of the physical constants appropriate to layer-type cloud. It has been shown elsewhere (Best 1951c) that in layer-type cloud $W < 0{\cdot}2 \times 10^{-6}$ g cm^{-3} in slightly over 50 per cent of measurements and that the predominant drop diameter is less than 12 μ in about half the measurements. Moreover a predominant radius of 6 μ (with $n = 3{\cdot}25$) corresponds to $b = 6 \times 10^{-4}/0{\cdot}9 = 6{\cdot}7 \times 10^{-4}$ cm (Best 1951b). If we assume also that the cloud temperature is 5°C and that the supersaturation is 0·01 per cent we have the following values for the constants :

$M = 18$, $D = 0{\cdot}226$ cm^2 sec^{-1}, $R = 8{\cdot}314 \times 10^7$, $\theta = 278$°A, $s = 10^{-4}$,
$p_\theta = 8{,}719$ dynes cm^{-2}, $W = 0{\cdot}2 \times 10^{-6}$ cm^3/cm^3, $b = 6{\cdot}7 \times 10^{-4}$ cm,
$g = 981$ cm sec^{-2}, $\mu = 1{\cdot}74 \times 10^{-4}$ g cm^{-1} sec^{-1}.

These lead to

$$G = 1{\cdot}535 \times 10^{-10} \qquad \frac{Wgb}{6\mu} = 1{\cdot}259 \times 10^{-4}$$

With these values Table I has been compiled to show f_n'/f, $(f_a' + f_d')/f$, $f_n'/(f_a' + f_d')$ and the fraction of liquid water comprised by drops smaller than k for various values of k and n.

TABLE I. FRACTIONAL RATE OF INCREASE OF NUMBER OF DROPS OF SPECIFIED SIZE OWING TO CONDENSATION OR TO COALESCENCE

$k =$	0·5	0·8	1·0	1·1	1·3	2·0
n			$10^4 f_n'/f$			
1	62	26	17	14	11	6·0
2	48	23	17	15	13	9·4
3	32	19	17	17	17	22
4	17	14	17	19	25	56
			$10^4 (f_a' + f_d')/f$			
1	− 1·6	− 1·2	− 0·85	− 0·62	− 0·11	1·8
2	− 1·6	− 1·1	− 0·54	− 0·10	1·1	9·4
3	− 1·8	− 1·4	− 0·61	0·12	2·7	30
4	− 1·8	− 1·5	− 0·75	0·32	5·4	84
			$f_n'/(f_a' + f_d')$			
1	− 39	− 21	− 20	− 23	− 94	3·3
2	− 29	− 21	− 32	− 150	12	1·0
3	− 18	− 14	− 28	140	6·5	0·74
4	− 9	− 10	− 23	60	4·7	0·66
			$1 - \exp(-k^n)$			
1	0·39	0·55	0·63	0·67	0·73	0·86
2	0·22	0·47	0·63	0·70	0·82	0·98
3	0·12	0·40	0·63	0·74	0·89	1·0
4	0·06	0·34	0·63	0·77	0·94	1·0

The first part of the table shows that the number of drops per unit volume of a iven size increases by about 0·2 per cent sec^{-1} owing to the condensation processes. 'he second part shows that the effect of coalescence is very dependent upon the drop ze. At the smaller sizes coalescence diminishes the number of drops by about 0·01) 0·02 per cent sec^{-1} but at the larger sizes coalescence leads to an increase in the umber of drops at a rate which reaches about 0·03 per cent sec^{-1} for $k = 1{\cdot}3$ and $= 3{\cdot}0$. The values of f_a' and f_d' have not been tabulated separately but it is of interest) note that the computations showed that Z is always positive (and so f_d' is negative). \t small values of k, Z is larger than $P/3k^2$ (and with the same sign) by a factor of to 20. For large values of k however Z diminished to zero and $P/3k^2$ became large nd negative.

The relative importance of condensation and coalescence is measured by the umerical values of the entries in the third part of the table and we see that unless exceeds 1·3, condensation is far more effective in determining the size distribution f the drops than is coalescence (in the conditions postulated). The last part of the able shows that the predominant influence of condensation (for $k < 1{\cdot}3$) applies to pproximately 90 per cent of the liquid water in the cloud even with the assumptions nentioned in the introduction.

Let us now consider conditions typical of a well-developed convective cloud. We note first that the signs of P, A_3 and Z depend only upon n and k so that changes n the values of θ, b and W will not affect the sign of any term in the table. Reasonable alues of the liquid water concentration and the predominant drop diameter are Best 1951c) $0{\cdot}3 \times 10^{-6}$ cm^3/cm^3 and 20 μ respectively, the latter value leading to $= 11{\cdot}1 \times 10^{-4}$ cm. We shall also assume $\theta = 268°A$, $\mu = 1{\cdot}69 \times 10^{-4}$ g cm^{-1} sec^{-1} nd $D = 0{\cdot}211$ cm^2 sec^{-1}, $p_\theta = 4{,}215$ dynes cm^{-2}. The choice of a suitable value for presents some difficulties but Howell's work (Howell 1949) suggests that in a vigorous onvective cloud $s = 1{\cdot}5 \times 10^{-3}$ may not be an overestimate. With these values for he constants the entries in parts 1, 2 and 3 of the table should be multiplied by 2·56, ·56 and 1·0 respectively. Of course it is coincidence that the first two of these factors hould be identical. The precise value of s is subject to considerable uncertainty. Nevertheless it seems reasonable to conclude that if the supersaturation in a vigorous onvective cloud is of the order $1{\cdot}5 \times 10^{-3}$ or higher, condensation will be more mportant than coalescence in determining the drop-size distribution. In fact coales- ence will be even less important than is indicated by the table owing to the assumption f unity for the collection coefficient of a falling drop.

5. The balance between condensation and diffusion

Having shown that the effect of coalescence on drop-size distribution is unimportant ompared with the effect of condensation it follows that when a steady state is reached he rate of variation of f owing to condensation must be balanced by an equal and pposite rate of variation owing to diffusion.

We now assume that f can be written in the form

$$f = \phi(r)\,\psi(z) \qquad . \quad . \quad . \quad . \quad (17)$$

When the steady state is reached we have $\partial f/\partial t = 0$ and so from Eq. (17)

$$\frac{1}{\phi}\frac{\partial \phi}{\partial t} = -\frac{1}{\psi}\frac{\partial \psi}{\partial t} \qquad . \quad . \quad . \quad . \quad (18)$$

Now $\partial\phi/\partial t$ can be replaced by $-\partial(\phi\partial r/\partial t)/\partial r$ and the usual derivation of the diffusio equation allows us to replace $\partial\psi/\partial t$ by $-K\,\partial^2\psi/\partial z^2$ where K is the eddy coefficient c diffusion with a value about 10^5 cm^2 sec^{-1} (and is assumed constant with height) Making these substitutions and combining with Eq. (2) we then get

$$\frac{1}{\psi}\frac{\partial^2\psi}{\partial z^2} = \frac{G}{K}\frac{1}{\phi}\frac{\partial}{\partial r}\left(\frac{\phi}{r}\right) \qquad (19$$

6. Solution of the equation

Eq. (19) governs the drop-size distribution in cloud. If z is measured vertically from the cloud centre towards the upper surface of the cloud (assumed to be of th layer type) the boundary conditions are

(a) $f = 0$ when $z = l$, where l is the semi-thickness of the cloud.

(b) $f = 0$ when $z = -l$.

Equating each side of Eq. (19) to $-A^2$ where A is a constant we get

$$\psi = B\cos Az + C\sin Az$$

where B and C are constants. The boundary conditions then give

$$0 = B\cos Al + C\sin Al$$

$$0 = B\cos Al - C\sin Al$$

Thus

either $\quad C = 0 \qquad Al = (2p+1)\frac{\pi}{2}$

or $\quad B = 0 \qquad Al = p\pi$

where p is an integer. The second alternative is unreasonable as it implies $\psi = 0$ ir the middle of the cloud. We therefore adopt the first alternative and since we want ψ to have the same sign for all values of z between l and $-l$ we put $p = 0$ thus getting

$$\psi = B\cos\frac{\pi z}{2l}$$

We then have from Eq. (19)

$$-\frac{\pi^2}{4l^2} = \frac{G}{K}\frac{1}{\phi}\frac{\partial}{\partial r}\left(\frac{\phi}{r}\right)$$

The solution to this equation is

$$\phi = Hr\exp\left(-K\pi^2 r^2/8l^2 G\right)$$

where H is a constant. Thus the full solution of Eq. (17) is

$$f = J\cos\frac{\pi z}{2l}\,r\exp\left(-Qr^2\right) \qquad (20$$

where

$$Q = K\pi^2/8Gl^2 \qquad (21$$

and J is an undetermined constant.

7. Cumulative size distribution of drops

If the drop-size distribution is described by Eq. (20) then at any point in the cloud the amount of water per unit volume comprised by drops having radius less than r

$$= J \cos \frac{\pi z}{2l} \frac{4\pi}{3} \int_0^r r^4 \exp(-Qr^2)\, dr$$

and the total amount of water per unit volume is represented by a similar expression but with the upper limit in the integral replaced by infinity. Thus the fraction of liquid water comprised by drops with radius less than r is

$$F = \int_0^r r^4 \exp(-Qr^2)\, dr \Big/ \int_0^\infty r^4 \exp(-Qr^2)\, dr$$

Making the substitution $Qr^2 = v$ we then get

$$F = \frac{\int_0^{Qr^2} v^{3/2} e^{-v}\, dv}{\Gamma(2{\cdot}5)} \qquad . \quad . \quad . \quad . \quad . \quad (22)$$

8. Comparison between the Incomplete Gamma function and empirical formula for drop-size distribution

In Pearson (1946) the function $I(u, p)$ is tabulated against u and p where

$$I(u, p) = \frac{\int_0^{u(p+1)^{\frac{1}{2}}} e^{-v} v^p\, dv}{\Gamma(p+1)}$$

From these tables $\log_{10} \log_e [1/(1-I)]$ has been plotted against $\log_{10} u$ for $p = 1{\cdot}5$. The result is shown in Fig. 1. It is clear that the points represented by circles (and which cover values of I from $I = 0{\cdot}1$ to $0{\cdot}9$) approximate reasonably closely to a

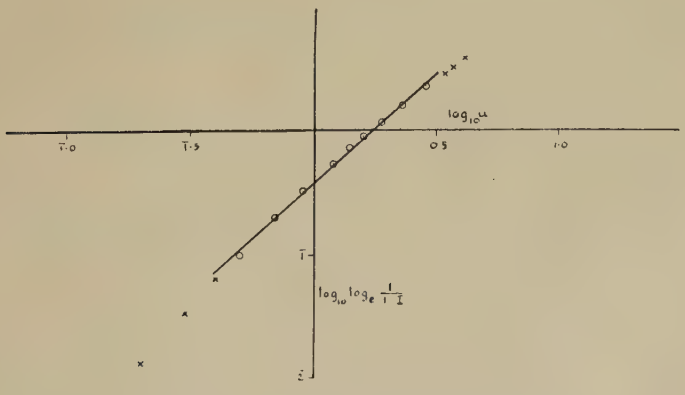

Figure 1. Variation with u of $I(u, 1{\cdot}5)\,\Gamma(2{\cdot}5) = \int_0^{u(2{\cdot}5)^{\frac{1}{2}}} e^{-v} v^{1{\cdot}5}\, dv$

straight line. The straight line drawn in the figure has been computed from these points by the method of least squares and yields the equation

$$\log_{10} \log_e [1/(1 - I)] = 1{\cdot}77 \log_{10} u - 0{\cdot}4253 \quad . \quad . \quad (23)$$

We may identify F in Eq. (22) with I in Eq. (23) provided we put

$$u\,(2{\cdot}5)^{\frac{1}{2}} = Qr^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad (24)$$

Doing this and writing x for $2r$ we find that Eq. (23) can be rewritten

$$1 - F = \exp\left[-(xQ^{\frac{1}{2}}/3{\cdot}317)^{3{\cdot}54}\right] \quad . \quad . \quad . \quad (25)$$

Eq. (25) is in the same form as Eq. (1) and we see that the theoretical value of the distributive index n is 3·54, in good agreement with the empirical value 3·25. It is rather remarkable that the value of this index should be independent of the numerical values of the usual meteorological parameters. This probably explains why no systematic variation in n was found in the previous paper (Best 1951b).

Comparing Eqs. (1) and (25) we see that

$$bQ^{\frac{1}{2}} = 3{\cdot}317 \quad . \quad . \quad . \quad . \quad . \quad . \quad (26)$$

Now from Eqs. (3) and (21) we have

$$Q = \frac{R\pi^2}{8M} \frac{K\theta}{Ds\, p_\theta\, l^2} \quad . \quad . \quad . \quad . \quad . \quad (27)$$

and it has been shown previously (Best 1951b) that the mean drop diameter is proportional to b. Thus the mean drop size is directly proportional to the thickness of the cloud and to the square root of the supersaturation and inversely proportional to the square root of the turbulence coefficient K. Substituting in Eq. (27) the numerical values (cgs units) $M = 18$, $\theta = 273°\text{A}$, $D = 0{\cdot}219$, $R = 8{\cdot}314 \times 10^7$, $p_\theta = 6{\cdot}108 \times 10^3$, $s = 10^{-4}$, $K = 10^5$, $l = 2 \times 10^4$ we get $Q = 2{\cdot}907 \times 10^6\ \text{cm}^{-2}$ and $b = 19{\cdot}5\ \mu$. This value of b is in good agreement with the values found from observations (Best 1951b) but of course a very considerable range of values is possible by varying the values of the individual parameters in Q. Two of these parameters in particular, K and s, are very difficult to assess. Nevertheless it does appear that the considerations outlined in Sections 5 and 6 do account reasonably well for the observed facts about drop-size distribution in layer-type cloud.

9. Acknowledgments

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A new method of stimulating convective clouds to produce rain and hail

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Summary

The theory of the formation of rain by coalescence leads logically to the suggestion that, if water drops of suitable size are sprayed into the base of a convective cloud, the drops will ascend in the upward air current, will grow by coalescence with the cloud droplets and may ultimately fall out as rain. Calculations show that under appropriate conditions the drops could attain a diameter greater than 1 mm and emerge from the base of the cloud from 20 to 60 min after water had been sprayed into it.

Eleven experiments have been carried out to test the possibility of the method. In 10 cases, distinct changes occurred following the introduction of a water spray into the selected cloud. In 4 of these, virga fell from the cloud and it dissipated soon afterward. In 4 cases, rain or hail appeared shortly after spraying. On only one occasion were no results observed.

The results suggest that the method might be useful for the artificial stimulation of rainfall, but further experiments are required to fully establish the mechanism.

1. Introduction

In a recent paper (Bowen 1950) a theory was developed to explain the formation of rain in convective clouds which do not reach freezing level. It was based on the assumption that, after the formation of cloud droplets in an ascending air current and their initial growth by condensation, some of the larger droplets continue growing by coalescence. Under suitable conditions these reach a size at which they come into suspension in the upward air current and, on further growth, fall out as raindrops. It has since been shown (Bowen 1951) that the process may also occur in clouds which build up above the freezing level.

A necessary condition for rain to form by the coalescence process is that a small fraction of the droplets in the lower part of the cloud should be larger than the average. These will fall relative to the smaller ones, will collide with those lying in their path and hence have opportunities of growing by coalescence. If such larger droplets do not exist, the cloud is unlikely to produce rain naturally by this process. *It may, however, be possible to stimulate it to produce rain if water drops of appropriate size are introduced artificially into the base of the cloud.* These drops should clearly be larger than those which already exist naturally but should be small enough to be carried upward in the ascending air current. A very similar proposal, derived from similar reasoning, has also been made by Ludlam (1951).

In the present paper some calculations are made of the extent to which water drops sprayed in the base of a convective cloud would grow by coalescence, and a description is given of experiments which have been carried out to check the results.

The method differs basically from that suggested by Langmuir and Schaefer (1947) using dry ice or silver iodide, which can be applied only to supercooled clouds. It also differs from a later suggestion of Langmuir (1948) that rain might be induced to fall by a chain-reaction process following the introduction of a few large drops into the top of a convective cloud.

2. Theory

It was shown in the previous paper (Bowen 1950) that the rate of growth of a droplet of diameter D falling relative to a cloud of other droplets is given by $dD/dt = wuE/2$, where w = cloud water content, u = relative velocity of the droplets, E = collection efficiency. Assuming the values of collection efficiency given by Langmuir (1948), this expression may be used to derive the trajectories of drops of known initial diameter which are introduced into a cloud of given characteristics, and which grow subsequently by coalescence. For simplicity it is assumed that the cloud is a uniform one, and consists of 20 μ-diameter droplets, has a water content of 1 g/m^3 and a uniform upward air current of 100 cm/sec. Actual clouds differ from this simple model in having drop sizes and a liquid-water content which increase with height and a non-uniform velocity structure ; but it was shown in the previous paper (Bowen 1950) that variation of the cloud parameters does not greatly influence the result.

Figures 1 and 2 show curves of height against diameter and height against time obtained in this way for drops of initial diameter 50, 75, 100 and 150 μ introduced 1,000 ft above the base of the cloud.

It is seen that drops which are initially 100 μ diameter emerge 22·5 min later with a diameter of 0·7 mm. This represents a growth of 7 times in diameter or 343 times in mass. Drops of 50 μ diameter, on the other hand, being carried higher, emerge after 50 min with a diameter of 1·35 mm, an increase of 27 times in diameter or almost 2×10^4 times in mass. Clearly, from the point of view of obtaining maximum growth, it is advantageous to inject the smallest possible drops, provided always that they are larger than the droplets already in the cloud and that the cloud has sufficient thickness to accommodate their trajectory.

It was also shown in the previous paper that of the cloud parameters likely to influence the rate of growth of drops growing by coalescence the mean upward air velocity was by far the most important. The above calculations have therefore been repeated for a variety of upward air currents, giving a series of curves similar to Figs. 1 and 2. They differ only in numerical values and are therefore not reproduced. From

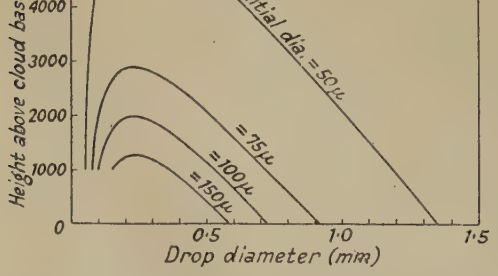


Figure 1. The growth by coalescence of drops sprayed 1,000 ft above the base of a cloud having the following characteristics : upward air velocity 100 cm/sec, water content 1 g/m^3, average cloud-droplet diameter 20 μ.

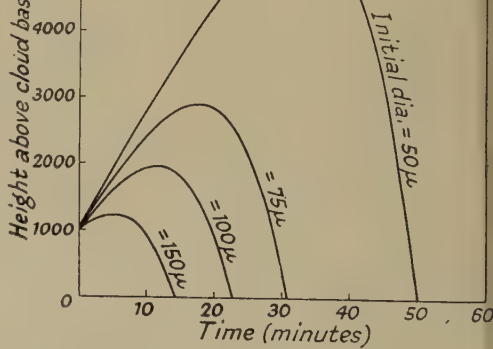


Figure 2. Curves of height against time for the conditions specified in Fig. 1.

hem a further curve, Fig. 3, is obtained giving the size of the drops which would be xpected to emerge as a function of the size of those which were originally sprayed nto the cloud, for upward air currents of 50, 100 and 200 cm/sec. It is evident that vithin the limits of validity of the calculations, the size of the emergent drops increases vith increasing air velocity and with decreasing diameter of the spray drops. As the pray drops are progressively reduced in size, however, the magnification will reach limiting value and tend to zero as their size approaches that of the cloud droplets hemselves, as shown in the dotted portion of the curve. Information about the collec- ion process at these sizes is not yet sufficiently accurate to determine precisely how he curve falls off.

Fig. 4, which is derived in a similar fashion, shows the time taken for drops to emerge from the cloud for different spray-drop sizes and for different vertical air velocities. From this curve it can be deduced that if drops of significant size are produced, they will appear, very approximately, from 20 to 60 min after spraying takes place.

It may be concluded therefore that, if water drops somewhat larger than cloud droplets are sprayed into the base of a convective cloud and the conditions outlined above are met, rapid growth of the spray drops will take place and they may eventually fall out as rain. Should the drops be carried in their ascent to regions where the temperature is sufficiently low to bring about freezing, the precipitation might take the form of hail.

It appears that the process would be an efficient one, since in the case of a cloud having an updraught of 200 cm/sec, the growth in mass of 50 μ drops injected into the base would be some 2×10^5 times. That is, for a total of 1,000 lb of water sprayed into the base of clouds, a fall of 100,000 tons could occur. This takes no account of further multiplication due to splitting of the drops, which might reasonably occur in clouds with greater updraughts.

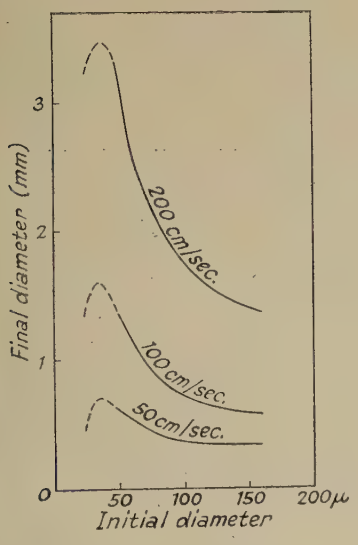


Figure 3. The size of drops which would be expected to emerge from a cloud as a function of the size of drops sprayed into it for vertical air velocities of 50, 100 and 200 cm/sec.

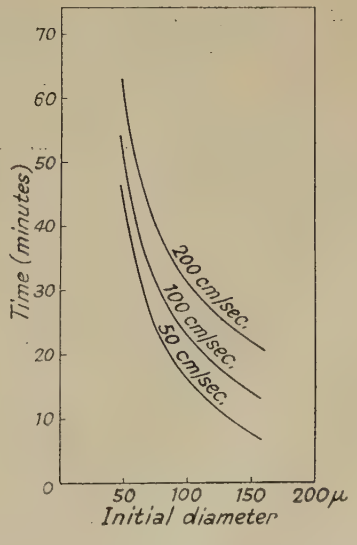


Figure 4. The time at which spray drops would be expected to emerge for upward air velocities of 50, 100 and 200 cm/sec.

3. Spray experiments

Experiments to check these expectations and to test the possibility of the method for inducing clouds to produce rain were carried out in the latter part of 1949 near Sydney, Australia. The experiments were performed in a Dakota aircraft of the R.A.A.F., fitted with a 66-gallon water tank and two horizontal spray bars each 6 ft long. The rate of flow was approximately 30 gal/min, so that when operated continuously the spray was laid along a line about 6 mi long. It was not possible to determine experimentally the distribution of drop sizes produced by the spray, but the best available information suggests that at a cruising speed of 120 kt the water emerged as a spray with a wide distribution of drop sizes having a median value of 50 μ.

In performing an experiment, a search was made for a cloud which as far as possible was :

(a) one of a number of similar clouds in a given area, so that a direct comparison could be made between the behaviour of the cloud and that of the surrounding clouds after water had been sprayed in its base, yet was sufficiently separated from the other clouds to make observation easy.

(b) in its early stages of growth, so that the spray drops could be introduced when the upward air velocity was highest ; that is, when the rate of growth was a maximum and not when the cloud had already reached its maximum development.

(c) not subject to a wind shear which might throw the drops out of the cloud at the top of their trajectories.

A temperature sounding was then taken outside the cloud and a spray run made through it, usually up or down wind, at a height of 1,000 ft above the cloud base. This height was chosen as that at which the cloud droplets would have already passed their initial stages of growth by condensation. If any water remained in the tank after the first run through, the aircraft turned, re-entered the cloud on a reciprocal track, and sprayed the remainder of the water. The aircraft then flew to and fro under the base awaiting results. In the absence of a reliable measuring device the diameter of any drops subsequently falling from the cloud was determined by careful observation of the size of the splashes made on the windscreen of the aircraft. These estimates depend on the judgment of the observer and are necessarily very approximate. They have been checked by flying through rain near the ground when an independent measurement of drop size can be obtained. It has been found that the smallest drops which can be identified individually on the windscreen are about 0·25 mm in diameter. When they exceed 1 mm they become audible hitting the skin of the cockpit. In the case of small hailstones, they are collected in sufficient number on excrescences around the windscreen for a good estimate of diameter to be made.

Clouds suitable for experiment were found on a total of 11 occasions during Sept., Oct. and Nov. 1949 and water was sprayed into them in the manner described. The experiments fall naturally into two classes, those in which the clouds were less than 5,000 ft thick and those in which they were more than 5,000 ft thick. The detailed results are given in Tables I and II respectively. It will be seen that in the 7 cases in which the clouds were less than 5,000 ft thick there were 6 occasions on which virga fell from the clouds, after which they dissipated, and one occasion on which no effects were observed. In the 4 cases where the clouds were more than 5,000 ft thick, however, there was a considerable fall of rain or hail within a short time of water being sprayed in the cloud base.

TABLE I. EXPERIMENTS ON CLOUDS LESS THAN 5,000 FT THICK

	Cloud ht. and temp.				Cloud thickness	Characteristics of the precipitation			Time to dissipation of cloud	Remarks
	Base		Top			Time to first appearance	Duration	Form		
1949	ft	°C	ft	°C	ft	min	min		min	
8 Sept.	1,500	+ 16	4,000	+ 14	2,500	23	4	Few 0·25 mm drops	Did not dissipate	
27 Sept.	2,500	+ 9	4,000	+ 7	1,500	6	6	Few 0·25 to 0·5 mm drops	15	
10 Oct.	7,000	+ 6	10,000	+ 2	3,000	22	5	Few 0·25 mm drops	36	
12 Oct.	6,000	+ 12	10,000	+ 3	4,000	—	—	—	Did not dissipate	Considerable wind shear
18 Oct.	6,500	+ 9	8,000	+ 8	1,500	5	8	Few 0·5 mm drops	14	
7 Nov.	8,000	+ 4	12,000	0	4,000	18	7	Few 0·5 mm drops	28	
25 Nov.	8,000	+ 4	12,000	− 3	4,000	10	5	Few 0·25 mm drops	27	

TABLE II. EXPERIMENTS ON CLOUDS MORE THAN 5,000 FT THICK

	Cloud ht. and temp.				Cloud thickness	Characteristics of the precipitation					Time to dissipation of cloud	Remarks
	Base		Top			Time to first appearance	Duration	Form	Intensity	Area		
1949	ft	°C	ft	°C	ft	min	min			mi²	min	
29 Sept.	5,500	+ 8	12,000	− 6	6,500	23	37	1 mm raindrops 1-2 mm hail	heavy	18	65	
30 Sept.	4,500	+ 9	12,000	− 6	7,500	23	24	1-2 mm raindrops 2-3 mm hail	heavy	24	50	Five lightning strokes from base of cloud to ground
20 Oct.	8,500	+ 9	14,000	− 7	5,500	43	15	rain and hail > 3 mm	v. heavy	1	61	
2 Nov.	3,500	+ 10	10,000	0	6,500	15	25	0·5 mm raindrops	light	6	Did not dissipate	

4. Experiments on clouds less than 5,000 ft thick

Dealing first with the 7 experiments listed in Table I, six of them followed a very similar pattern. Virga fell from the clouds at times varying from 10 to 25 min after they were sprayed with water. The aircraft flew through the falling drops and found that they were very few in number and ranged from 0·25 to 0·5 mm in diameter. They almost certainly evaporated before reaching the ground. Shortly after the appearance of these drops, five of the six clouds thinned out and disappeared in the course of a few minutes.

The behaviour of the treated clouds was significantly different from that of surrounding ones. The latter did not produce virga and did not dissipate within the next hour. The results may therefore be ascribed to the effects of the water spray.

In the remaining case, that of 12 Oct., no visible effects were obtained. No precipitation fell from the cloud, and it did not dissipate. This was the one case in which a considerable wind shear existed, and it is therefore possible that the spray drops were carried out of the cloud soon after they were injected.

5. Experiments on clouds more than 5,000 ft thick

Dealing now with the 4 experiments on thicker clouds listed in Table II, three of these gave heavy falls of rain and hail, and the fourth a fall of light rain with no hail. Each one has points of interest, and they are described separately below.

(*a*) *Experiment of 29 Sept.* 1949

On the afternoon of 29 Sept. towering cumulus clouds were building up and decaying over the mountains 20 to 30 mi N. of Richmond, N.S.W. One of these was chosen for the experiment, the cloud base being at 5,500 ft and the temperature at that level + 8°C. The wind at all heights was from the west, being light on the ground and increasing slightly with altitude. The top of the cloud was well rounded and at no time during the experiment gave the appearance of being glaciated. At the time the spray was introduced, the top of the cloud was below 10,000 ft. It was building rapidly, however, and was estimated to have reached 12,000 ft at the time of maximum development. The clear air temperature at that height was — 6°C.

Two runs of about 1 min each were made through cloud at a height of 1,000 ft above the cloud base, during which 66 gal of water were released from the spray tank. After the spray runs the aircraft returned to the base of the cloud, and flew to and fro under the base. Ten minutes after the spray runs, the bottom of the cloud became very dark and threatening and the base had lowered to 4,500 ft. After 23 min precipitation was seen to be coming from the whole of the bottom of the cloud and it fell to the ground in a clearly defined column over an area estimated to be 6 mi long by 3 mi wide. The aircraft flew through it several times and it was found to consist of raindrops approximately 1 mm in diameter and hail 1 to 2 mm in diameter.

Forty-four minutes after the spray had been released the base of the cloud had risen to 6,000 ft and was more diffuse than earlier. The cloud was still raining heavily and fractocumulus began to form beneath. After 52 min the rain was noticeably lighter and it ceased entirely 60 min after the spray runs. Thereafter the cloud broke up rapidly and dissipated within the course of a few minutes.

While the experiment was in progress the majority of the surrounding clouds emained substantially unchanged. Three clouds within 50 mi did give rain, however, ut their characteristics were markedly different from that on which the experiment vas performed. The tops of these clouds became glaciated and the rain from them vas less intense and rather more widespread in area. They also lasted longer and had not lispersed by the time the aircraft left the area.

(*b*) *Experiment of* 30 *Sept.* 1949

Another experiment was performed on a similar cloud in the same area on 30 Sept. nd the sequence of events was almost identical. Heavy precipitation began to fall fter the same interval of time, namely 23 min after water was sprayed into the base, ver an area 6 mi long by 4 mi wide. The raindrops were estimated as between 1 and 2 mm in diameter and the hail from 2 to 3 mm in diameter. The aircraft flew through he precipitation several times, but on the third occasion a strong downdraught was ncountered under the eastern half of the cloud and the aircraft had difficulty main- aining height. The rain and hail continued to fall heavily to the ground for 24 min, fter which it ceased abruptly and the cloud dispersed.

About 5 min after precipitation started there was a lightning stroke from the centre f the cloud to the ground, and four other strokes occurred before the rain and hail eased off. This is the only occasion on which this phenomenon has been observed ollowing the introduction of water drops into the base of a cloud. If it is repeated n future occasions it might prove of great significance to theories of the generation f electricity in clouds.

As in the previous experiment, the surrounding clouds did not give rain and re- nained largely unchanged in form. While rain was falling from the cloud experimented upon, however, an icy canopy spread over the area from a cumulonimbus cloud about 100 mi to the west. When the rain from the treated cloud had ceased, this canopy ell into the top of the clouds in the area and they also commenced to rain. The cloud n which the experiment was performed had by this time dispersed completely leaving clear space in an otherwise congested atmosphere.

(*c*) *Experiment of* 20 *Oct.* 1949

The experiment of 20 Oct. was generally similar to the two previous examples xcept that the cloud base was higher and the precipitation did not fall until 43 min fter the spray run was made. The fall was in the form of a narrow column approxi- nately 1 mi square and was so heavy that it was not considered advisable to fly through he centre of it. The aircraft flew through the edges and found that it consisted of arge raindrops and hailstones which exceeded 3 mm in diameter. The fall continued or 15 min and then stopped abruptly. This was followed by dissipation of the cloud, s in the previous cases.

On this occasion there were several other clouds in the vicinity which also pro- duced rain and hail, and this particular experiment must therefore be regarded as of doubtful significance.

(*d*) *Experiment of* 2 *Nov.* 1949

The conditions under which the fourth experiment was performed, and the esults obtained, were considerably different from the three earlier ones. The cloud

concerned was 30 or 40 mi to sea, and after water had been sprayed into the base ligh rain fell from it over an area of approximately 6 square mi. There was no hail, as would be expected from the fact that at no time did any part of the cloud attain a temperatur below freezing. Unlike the previous cases, the cloud did not dissipate when the rai stopped falling. It began to thin out, but built up again to its original form shortl afterwards.

The cloud was one of a cloud street some 100 to 200 mi long lying parallel with the coast. The one on which the experiment was performed was the only one of these to produce rain. The nearest rain which fell naturally in the area was from an entirely different cloud formation some 50 mi further out to sea.

6. Discussion of results

It is seldom possible in a single experiment on cloud modification to be certain whether the effects observed are in fact the result of the experiment, or would have occurred in any case. The results of a series of experiments can be of real significance, however, if the sequence of events follows a consistent pattern. The observations of the present series show certain differences which are associated with differences among the clouds used for the experiments, but there are several ways in which they follow a set pattern :

(i) With a single exception, the clouds chosen for experiment behaved in a manner which was different from that of surrounding clouds immediately following water droplets being sprayed into their base.

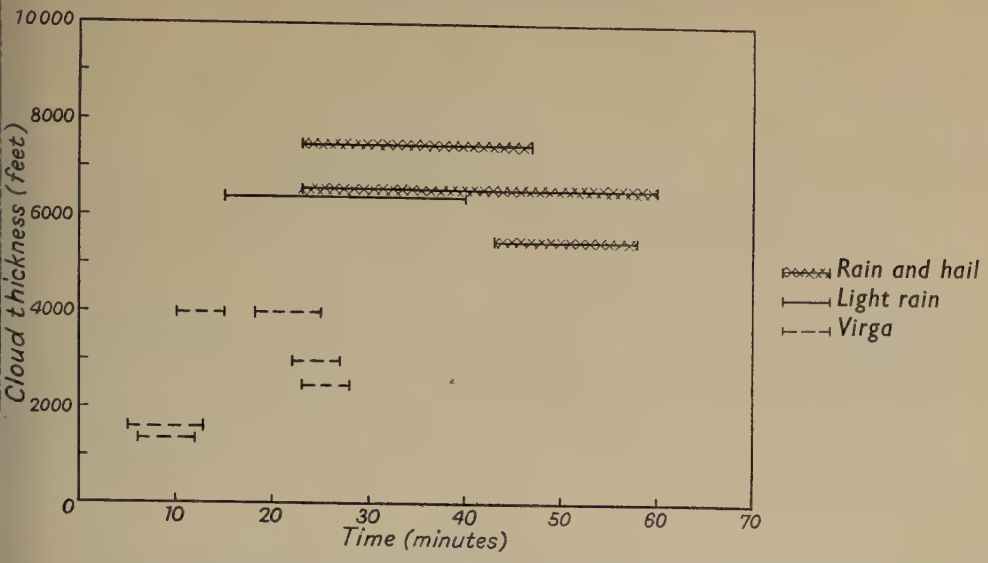
(ii) In the 4 cases in which precipitation fell to the ground, the observed results were in qualitative agreement with expectation. The time at which precipitation first appeared, and its duration, were close to the times which would be expected from the theory. The size of the drops emerging were also of the order of magnitude expected.

(iii) Finally, if the time of the first appearance of precipitation and its duration are plotted against cloud thickness as in Fig. 5, a scatter diagram is obtained which shows a consistent increase in time with thickness of cloud. If the appearance of the precipitation were not connected with the introduction of water drops into the cloud, these times should bear a random relation to the cloud thickness.

From the consistency of these results and the way in which they conform to expectations it may be concluded that there is a high probability that the observed effects were due to the spraying of water drops into the cloud base.

7. Conclusion

It appears that the process of spraying water drops into the base of convective clouds during their growth stage may prove an effective method of stimulating them to produce rain or hail. If the method can be substantiated, it might prove important for modifying the distribution of rainfall in those parts of the world where rain tends to form by coalescence in convective cells, rather than by the Bergeron process.



gure 5. Experimental observations of the time of first appearance and the duration of precipitation plotted against thickness of the cloud experimented upon.

The experiments described cannot be claimed to have established the method ith certainty, and further experiments will be necessary. It is proposed to continue em with the broad objective of :

(a) building up statistical data from which the effects can be defined with more certainty.

(b) obtaining direct evidence, by radar and other means, of the history of the spray after it enters the cloud.

(c) obtaining a quantitative relation between the size of the drops sprayed into the cloud and the size of the emergent raindrops, as related to quantities like vertical air velocity, cloud water-content etc.

(d) establishing the optimum conditions for practical rain-making purposes.

Acknowledgments

The experiments were carried out in aircraft of the R.A.A.F. Aircraft Research nd Development Unit based at Richmond, New South Wales. Acknowledgments e due to the crew of the aircraft for their great assistance in the conduct of the experi-ents and particularly to the pilot, Flt. Lt. Lynch, who carried out the flights with eat competence under conditions which were not entirely devoid of danger.

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A new method of measurement of cloud-water content

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Summary

A new method of measurement of the liquid-water content of clouds is described. It depends on the change of resistance of a paper tape on being moistened by cloud droplets which impinge directly on it. A continuous and permanent record of the liquid water can be obtained, and variations over distances of a few hundred feet can be detected. Measurements have been made using the technique on a number of cumulus and stratocumulus clouds, some characteristics of which are described.

1. Introduction

The liquid-water content of a cloud is an important parameter in describing its characteristics, and has been the subject of study for some considerable time. Measurements have been made from mountain tops by Köhler (1927), Houghton and Radford (1938) and many others, and recently investigations have been made from aircraft. The latter type of measurement has been directed primarily with a view to understanding the conditions necessary for aircraft icing to occur. It is also the only method that offers a reasonable possibility of obtaining the data on the distribution of liquid water throughout any one cloud that is necessary in the formulation of theories of rain formation or in checking theories of air entrainment.

Three different methods of measurement from aircraft have been reported as successful. Diem (1942) collected discrete samples of the cloud droplets on prepared slides and subsequently examined them under a microscope. The liquid-water content was obtained by calculation from the measured drop-size distribution and the exposure time. Diem (1942), and Vonnegut *et al.* (1946) describe a method depending on the amount of water collected, often in the form of rime, on a series of cylinders which are subsequently weighed. In a third method, due to Vonnegut (1949), the droplets are collected on a porous plug and then transferred to a capillary tube where a measurement is made of the movement of a meniscus as a function of time.

The first method suffers from errors due to sampling, since slides can only be exposed at relatively infrequent intervals, and then only for a very short time. The second technique is only capable of giving the average water content over a relatively long path. The last method is the only one which offers prospects of determining the variations within any traverse of a cloud, but even so the response is somewhat slow, and the method does not readily lend itself to continuous recording of the liquid-water content of the cloud.

In order to avoid some of these defects, a new method of measurement was developed by the authors, which is embodied in an instrument to be described below. This instrument is being used in a programme of research into the physical properties of clouds, particular interest being taken in the variation of liquid water both horizontally and vertically throughout the cloud. The instrument is capable of producing a continuous and permanent record of the liquid-water content of clouds traversed by the aircraft in which it is installed. It also indicates variations over distances of only a few hundred feet.

2. Description of instrument

The instrument depends for its operation on the fact that the electrical resistance of paper is a function of its moisture content. A paper tape is moved past a narrow slit open to the airstream, where it is wetted by the cloud droplets. After a short delay to allow the paper to absorb the droplets collected on its surface, the resistance across the tape is measured and recorded on a recording milliammeter. A sketch of the instrument is shown in Fig. 1.

A 100 ft roll of paper, 1 in. wide, is contained in a drum and is pulled over rollers driven by means of a motor whose speed can be varied to give tape speeds ranging from 2 to 24 in./min. The tape then passes under a slit 0·1 in. wide placed on the forward end of the instrument cover, which is in the form of a half-cylinder 0·375 in. in diameter. Its resistance is measured between two contacts, 0·4 in. apart and placed about 2 in. from the slit. Before disposal of the paper in the slipstream it passes over a roller carrying a cam which actuates contacts, thus operating a counter which indicates the amount of tape which has been used. In the event of the aircraft passing through heavy rain, or when the instrument is not in use, a motor-driven shutter can be used to close the inlet slit.

The instrument is not satisfactory for use in moderate or heavy rain, as the slit becomes covered with water. In any event, rain droplets break up on impact and much

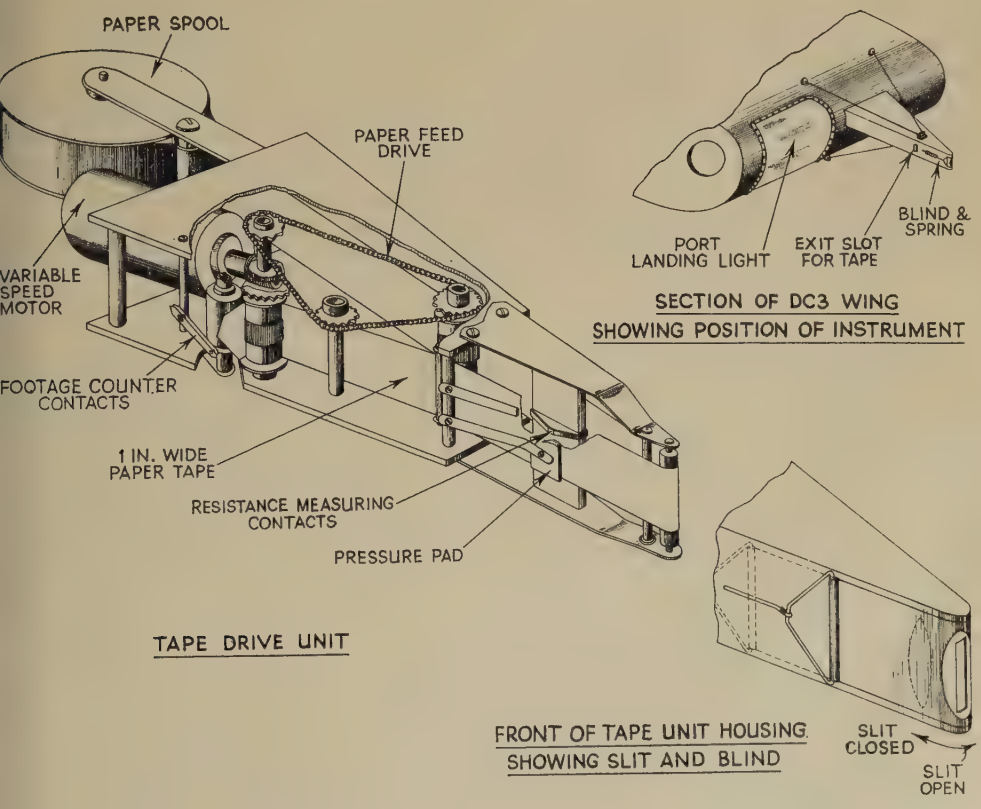

Figure 1. Sketch of instrument showing installation on aircraft wing.

water is lost by splash, making any readings unreliable. It is unlikely that the instrument could be used at temperatures much below 0°C, although readings which appear to be satisfactory have been obtained at temperatures down to — 2°C.

3. Characteristics of the paper tape

The paper tape was cut from rolls of special recorder paper of the type used in electrochemical recording. It was not specially treated in any way before use. All the paper used in the experiments made by the authors came from a single roll.

An investigation indicated that the paper was uniform throughout the roll in its reaction to wetting.

The resistance of the paper when dry is of the order of 1,000 MΩ as measured between the instrument contacts. It is of course affected by water vapour. However, the paper only takes up vapour slowly and the change in resistance due to sudden immersion in a saturated atmosphere at 20°C is relatively slow, a period of 5 min being required for the resistance to drop to 100 MΩ. This value of resistance was the highest considered to be significant in the subsequent use of the instrument in cloud measurement.

The reaction of the paper to liquid water was tested by spraying. Laboratory-produced sprays having mean drop diameters of from 10 μ to 100 μ were investigated to determine the effect of droplet size. Within the accuracy of the measurements, a given weight of water per unit area of the paper always resulted in the same resistance, irrespective of the size of the droplets. Larger drops, of rain-drop dimensions, might be expected to produce different results, due to uneven wetting of the paper.

The temperature coefficient of resistance of the paper was measured at two typical degrees of wetting and found to be of the order of — 2·5 per cent per °C. However, even at that part of the calibration curve most sensitive to resistance, a change in resistance of 2·5 per cent corresponds to an apparent change in water content of only 0·5 per cent. Hence it is unlikely that temperature variations will cause errors in water content of more than 5 per cent.

4. Calibration

In common with all other instruments for cloud measurements, this instrument is incapable of collecting all of the smallest droplets present in clouds. From Langmuir and Blodgett (1946) we can calculate the ' collection efficiency,' or fraction of droplets caught, at the stagnation point of a cylinder of the same diameter as the forward end of the instrument. This is a function of droplet size and air speed, as is shown in Fig. 2. If we consider the volume of water collected, rather than the collection efficiency for droplets of only one size, then the average collection efficiency in a typical cloud (50 per cent volume diameter = 25 μ) is approximately 90 per cent at aircraft speeds.

Calibrations were made in a small vertical wind tunnel about one foot in diameter, in which water droplets were injected from a spray gun operated by compressed air. The air velocity used was 10 kt, or about 500 cm/sec. The average droplet size in the spray was 50 to 100 μ and the collection efficiency approximately unity. The liquid-water content of the spray was measured either by means of a capillary collector of the type used by Vonnegut (1949), or by sampling it on a small paper strip which was weighed before and after exposure. For any given tape resistance the methods agreed only to about 10 to 20 per cent of the liquid-water content measured.

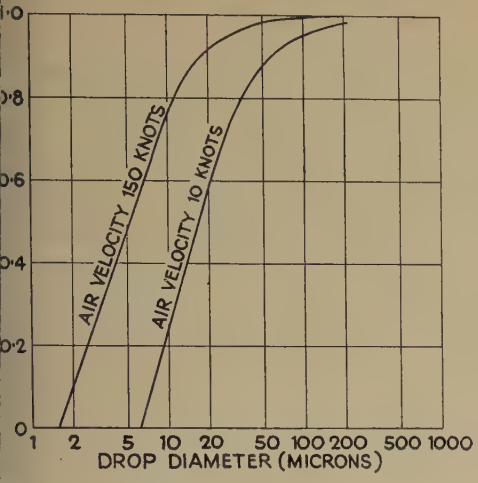

Figure 2. Collection efficiency at stagnation point of cylinder of 3/8 in. diameter.

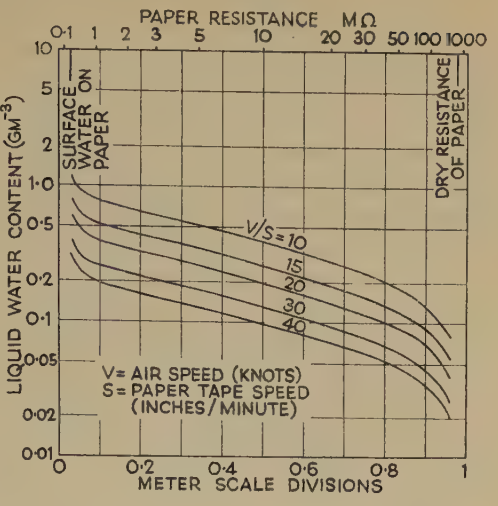

Figure 3. Calibration curves of instrument.

In transforming the laboratory calibration to flight conditions it has been assumed that the resistance of the tape depends only on the amount of water collected per unit area, which for a given liquid-water content of spray is a linear function of the ratio of air speed to speed of movement of the paper tape past the slit. This assumes that the collection efficiency is constant, which was nearly the case since larger drops were used at the slower air speeds available in the laboratory, and that the velocity of impact of the droplets on the tape has no effect. As far as could be ascertained these assumptions appeared justified.

The final calibration used is shown in Fig. 3. It is considered to be reliable to ± 20 per cent in water content for cloud temperatures of 10°C. It has been used, however, without any correction for temperature, and hence the results quoted below should not be considered accurate to better than 25 per cent.

5. Results

The instrument was installed near the landing light on the leading edge of the port wing of a Douglas DC3 aircraft, in a position well clear of the propeller and forward of the leading edge. Measurements, chiefly on cumulus and stratocumulus clouds, have been in progress for a few months. The normal flight procedure has been to traverse the clouds horizontally at 500 ft intervals starting from the top and travelling downwards to the base, readings being taken continually. As far as possible isolated clouds have been selected for study; thus the instrument starts from clear air at the commencement of each traverse. The instrument was seldom in cloud for longer than two or three minutes at a time and hence results were not markedly affected by the presence of water vapour. Whenever the nominal 'dry' resistance of the paper dropped to 100 MΩ on exit from the cloud, a few feet of fresh paper were run through until the resistance became greater than 500 MΩ before the next traverse. After exit from the cloud base, the aircraft returned to the top of the cloud again to allow observations to be made of the stability of the cloud, and wherever possible one or two

check points have been selected for measurement during a second descent. The total time of a measurement varied from one cloud to another, but was of the order of 30 min.

The results of a series of flights through a small cumulus cloud are given in Fig. 4, which is a reproduction of the actual record taken. In only some of the traverses is the water content roughly constant throughout the traverse ; in the others there is a series of peaks. In examining a number of these records it has been observed that the value of water content having the most significance appears to be the average of these peak values during that part of the traverse in which the aircraft is in the main body of the cloud. This value of cloud-water content has been taken for each traverse from the record of Fig. 4 and plotted as a function of height in Fig. 5. Two other typical graphs of water content versus height are shown in Fig. 6.

It is seen that a characteristic of the cumulus and stratocumulus clouds examined is the steady rise in water content from the base to just below the top, followed by the sudden decrease at the top. The rate of increase of liquid water with height is considerably less than the figure of 0·7 g/m^3 per 1,000 ft which might be expected if the

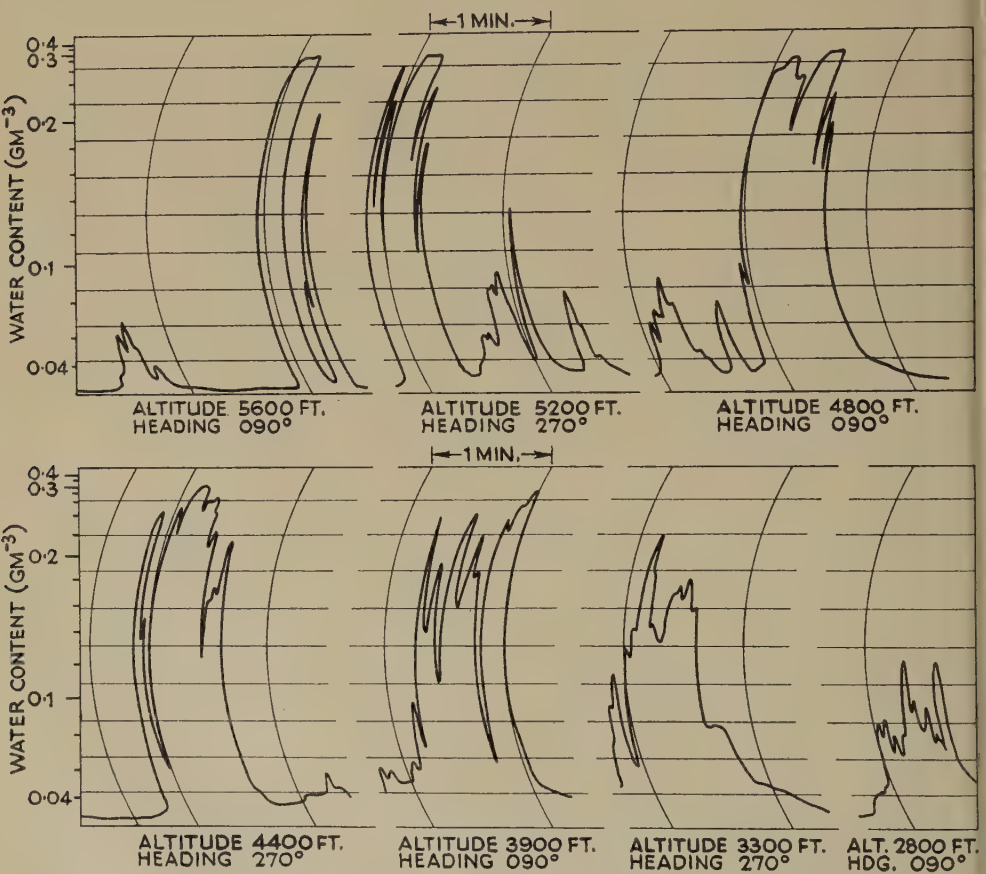

Figure 4. Flight record 30.1.51 ; measurements in a small cumulus cloud free from rain. General cloud situation : cumulus and stratocumulus 2-4/8, base 3,000 ft, tops up to 7 to 8,000 ft. ; altostratus 8/8, base 10,000 ft ; intermittent light rain ; freezing level 14,000 ft ; turbulence slight to moderate. (1 min on trace $\simeq$ 2½ mi).

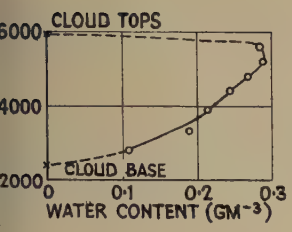

Figure 5. Water content as a function of height for flight on 30.1.51.

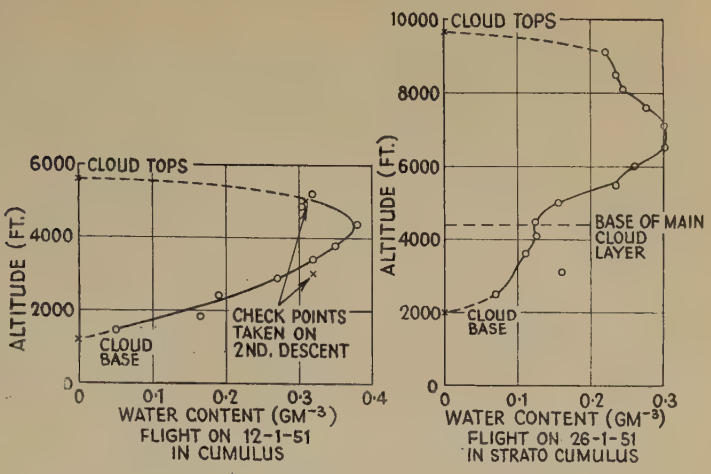

Figure 6 (a) and (b). Typical graphs of water content as a function of height.

air rose adiabatically. This effect has been observed by other workers and could be accounted for by entrainment of air from outside the cloud, or by relative motion between the cloud and the air mass in which it is formed. The values of water content obtained with the instrument are in moderate agreement with previously published values for stratocumulus and small cumulus clouds. Few previous measurements, however, have been made throughout the vertical extent of the cloud, nor indeed have their authors given the height above cloud base at which they were taken.

The second feature characteristic of all the clouds so far examined is the rapid fluctuation of water content throughout any traverse. The instrument does not respond fully to the most rapid fluctuations, the main limitation being the recording milliammeter, which takes nearly 3 sec to reach equilibrium conditions after a sudden applied change. The variability of water content is clear evidence of the need for an instrument of this type for cloud measurements, and of the doubtful significance of measurements involving either discrete samples or long-term averages of conditions in the cloud.

6. Conclusions

The instrument described makes possible rapid measurement and recording of the fine structure of the water content of clouds traversed by the aircraft in which it is installed. While the absolute calibration of the tape resistance as a function of water content is not as exact as may be desired, the relative figures of water content are thought to be of value. As yet no attempt has been made to draw any conclusions as to the meteorological significance of the measurements that have been made. These measurements, together with investigations of other properties of the clouds under examination, are still in progress.

Acknowledgments

The authors wish to acknowledge the interest displayed in and the encouragement to the project given by Dr. E. G. Bowen, and the help of Mr. K. Davidson in the construction of the instrument. Mr. G. A. Day was responsible for the initial investigations

of the properties of paper, wet by fine spray, which led to the adoption of the present method of measurement of cloud-water content, and also for early developmental work on the instrument. The flights were made at first in a Royal Australian Air Force aircraft operated by a detachment of the Aircraft Research and Development Unit R.A.A.F. and later, in a specially equipped aircraft owned and operated by Qantas Empire Airways Ltd.

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551.555.9

Mountain-gap winds ; a study of surface wind at Gibraltar

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(Manuscript received 17 May, in revised form 10 October 1951)

Summary

The surface wind in the Straits of Gibraltar is determined by the interplay of its inclination to blow towards the end where surface pressure is lowest when the air is stable and its tendency to follow the upper wind if convection is active. The importance of cold pools of air lying below mountain-top level is discussed and various kinds of wind reversal are described.

1. Introduction

This paper is the outcome of a study made at Gibraltar during 1943-4, whose purpose was to explain and predict changes of wind direction. They were important because there was a large volume of air traffic and only one runway, lying E.-W., and those results thought to be of general interest are gathered here.

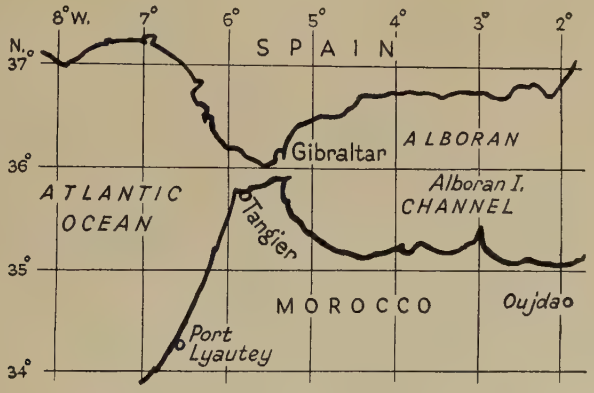

Figure 1. The Straits of Gibraltar.

The Straits of Gibraltar are the only sea-level gap in many thousand miles of high mountains, and are many hundreds of miles from the Toulouse and Rhone valley gaps. It is to be expected that many wind features found in mountain gaps will display themselves *in excelsis* in the Straits. This is indeed so, and the site is ideal for the study of these phenomena for there they dominate all others. Their understanding will lead to explanations of observations at other places where the features are more subdued.

2. Features of the problem

Distinction is first made between occasions of great static stability and others. The stability may be in the form of a marked inversion just below mountain-top level, or an almost isothermal layer up to that height, and though observed at all times

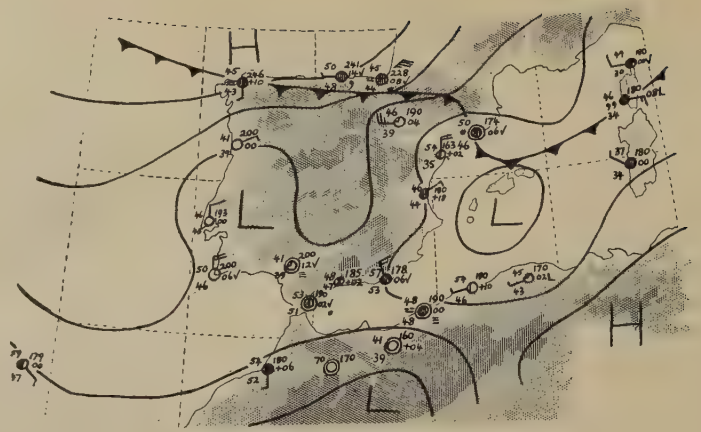

Figure 2. 17 March 1944, 0700 GMT.

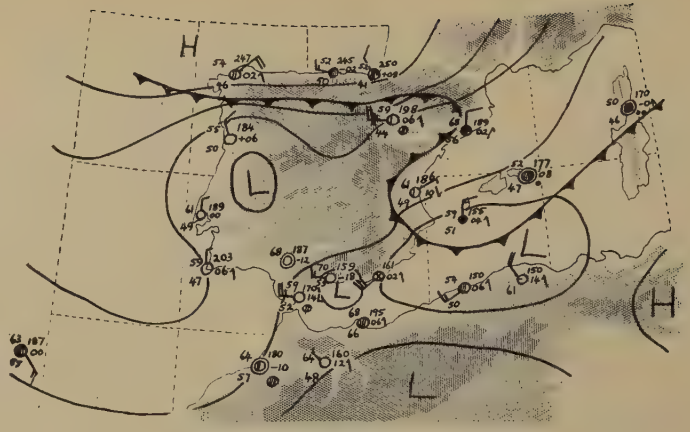

Figure 3. 17 March 1944, 1300 GMT.

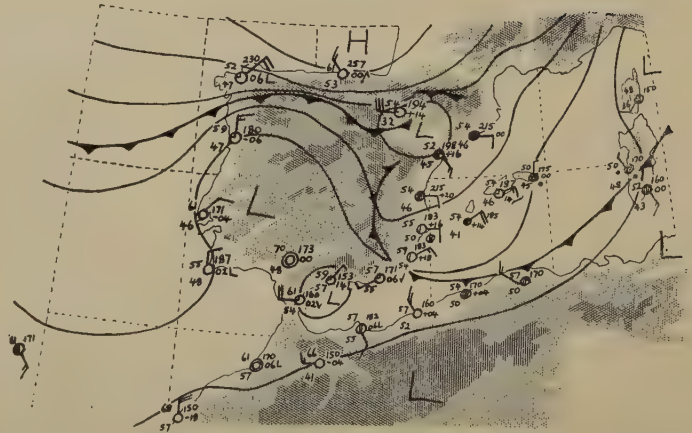

Figure 4. 17 March 1944, 1800 GMT.

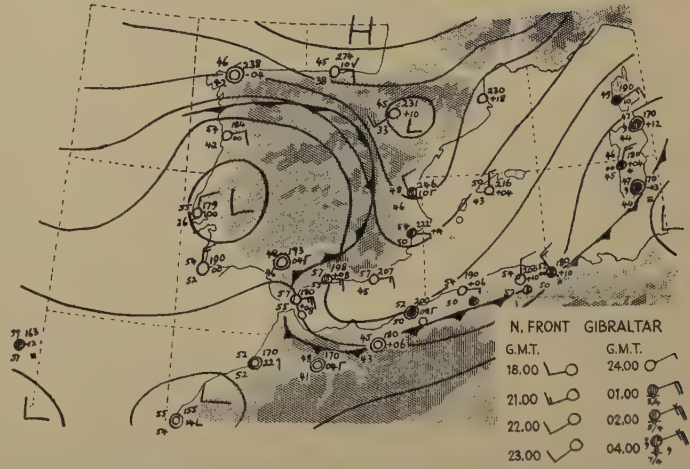

Figure 5. 18 March 1944, 0100 GMT, with hourly observations at North Front aerodrome, Gibraltar. (The observations plotted on the charts

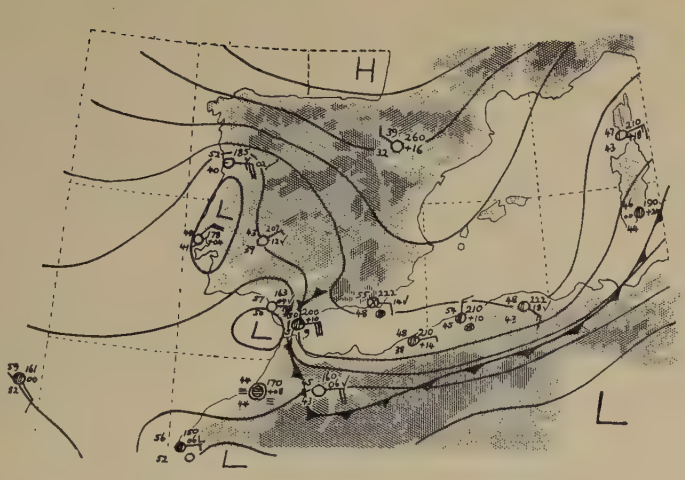

Figure 6. 18 March 1944, 0700 GMT.

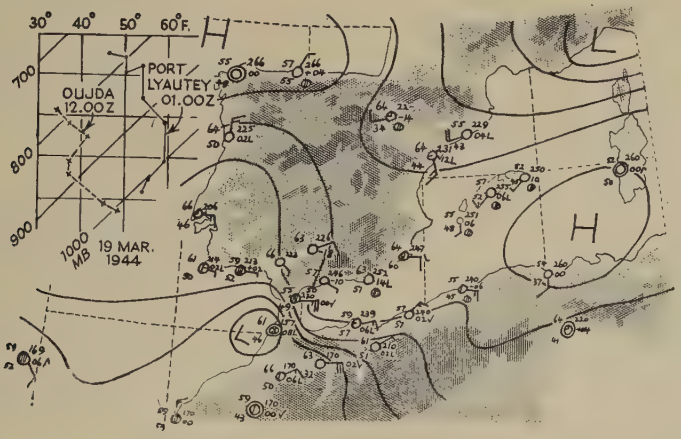

Figure 7. 19 March 1944, 1800 GMT, showing how little the situation at the mountain gap changed once the cold air had filled the western Mediterranean. The small amount of cold air passing through the straits and other mountain gaps does not alter the temperature difference between the air masses on the two sides of the mountains. The temperature difference is illustrated by the radio-sonde ascents for Oujda and Port Lyautey.

Figures 2-7. An example of the flow of shallow, cold air into the western Mediterranean.

of the year is most common in the summer, particularly in July. The other extreme common in winter months, is when considerable convection is active from the surface

A feature of the surface wind which is most noticeable in the cases of great stability is that it blows towards low pressure. The pressure at Tangier is frequently said to ' read low ' ; and this is easily understood as a necessary accompaniment of more or less horizontal motion through the gap. The wind speed is understandably observed to be greater in the narrows and is associated with a minimum of pressure, but we must explain how this minimum is produced for it is not a consequence but a pre-requisite of the nearly horizontal motion.

The air converging towards the narrows follows fairly smooth streamlines but when it emerges from the gap the pattern is not reversed. The air does not readily fan out up a pressure gradient to a lower velocity but rather tends to emerge as a jet with large standing eddies which can be observed in the surface isobars, or smaller moving ones which are observed by aircraft as regions of very changeable wind, on the sides of the jet. The flow through the narrows is not reversible and so there must be a net fall of pressure along the streamlines. It is not necessary to understand the details of the irreversibility in order to see that it is the reason for the undoubtedly observed flow of the air towards the low-pressure side of the gap, and why purely horizontal motion does not occur unless there is a pressure excess on one side or the other.

3. The influence of cold pools of air

Palmén (1951 Symons Memorial Lecture) has described how cold air moving southwards behind a depression subsides so as to occupy perhaps only the lowest 2,000 to 5,000 ft by the time it reaches 35°N., say. When the flow of this cold air is restricted by mountain ranges the surface weather is dominated by the flow through the mountain gaps and along precipitous coastlines. The upper surface of the cold air may be sloping, and whether it is sharp or diffuse makes no difference, so that the wind field in the cold air often bears almost no resemblance to that above. The motion of the cold air is determined by the topography and is suggestive of a lake of dense fluid flowing through the gaps in mountain ranges, and filling up the hollows. Figs. 2-7 show successive positions of a cold front in a typical case ; and the arrival of the cold air at one or the other side of the Straits of Gibraltar will determine the direction of the wind there. The cold air is responsible, in the absence of strong gradients of pressure in the air above, for the excess of pressure on the side of the mountain gap where it lies, and it ' leaks ' through the straits so long as the air below mountain-top

TABLE I. Upper winds (in degrees and mi hr^{-1}) at Gibraltar, 17 March 1944, just before the reversal of the surface wind to easterly

Height in ft	0930 hr		1520 hr		2210 hr	
Surface	—		270	17	270	05
500	—		270	14	270	07
1,000	230	09	280	17	330	06
1,500	—		290	20	035	07
2,000	290	02	285	13	060	07
2,500	—		280	15	060	09
3,000	270	06	315	02	090	11
4,000	—		310	07	085	05
5,000	360	09	350	06	130	06

evel on the other side is warmer. The height of the inversion or stable layer that caps the cold air must be lower on the lee side of the mountain gap, and this is how the pressure fall through the gap is maintained. If there are no great vertical accelerations the pressure minimum in the straits near Tangier is probably under a minimum of the depth of the cold air.

The lee depressions so often observed to appear behind mountain ranges at the approach of a cold front are thought to be simple manifestations of the temperature difference on the two sides at lower levels. The absolute fall of pressure is due to an upper trough advancing with the cold front but unimpeded by the mountains. These depressions can be seen in Figs. 2-7.

In Fig. 8 is an example of the shallow cold air coming from the west. In Fig. 9 the upper-air temperatures at the time show the great stability and Table II shows winds measured by pilot balloon. The change of wind at the top of the cold air is

Figure 8. Typical surface-pressure distribution for a shallow westerly wind. (Pressure and winds only). 23 Jan. 1944, 1800 GMT. Cold air mass to the west flowing through the straits. See also Fig. 9 and Table II.

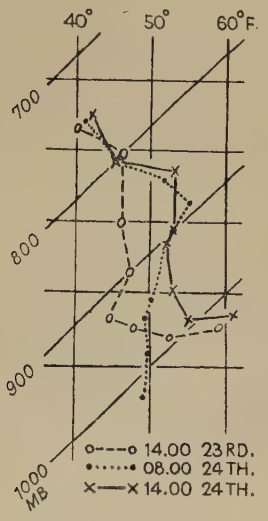

Figure 9. Upper-air temperatures showing very stable layer below mountain-top level. See Table II.

TABLE II. Upper winds (in degrees and mi hr^{-1}) at Gibraltar, 23 to 24 Jan. 1944, showing the marked change above the stable layer and above the local mountains. Figures in italics indicate the region of the very stable layer

Height in ft	23 Jan. 1140 hr		23 Jan. 1700 hr		24 Jan. 1335 hr		24 Jan. 2100 hr	
Surface	310	15	280	20	300	20	300	25
500	300	14	290	17	300	23	300	22
1,000	300	15	290	27	300	28	290	27
1,500	260	14	270	16	*300*	*20*	*290*	*24*
2,000	*250*	*12*	*260*	*14*	250	12	260	08
3,000	200	12	210	04	010	04	310	03
4,000	190	10	080	10	270	03	360	03
5,000	170	10	120	08	360	03	310	02

most marked. There is unlikely to be any very consistent relationship between the surface isobars round a mountain gap and the upper wind when the distribution of shallow cold air is determining the low-level flow. During the period of the observations of the upper wind in Table II there was no noticeable change in the surface-isobar distribution for a distance of 400 mi round Gibraltar, yet the upper wind varied quite a lot.

To forecast wind changes due to the arrival of cold air, the motion of the cold front must be carefully followed. All meteorological elements must be used to locate the front because diurnal variations may often mask changes that occur at its passage.

4. Changes in pressure distribution

Shallow cold air may often determine the pressure field but there are many occasions when there is no marked difference in the air masses on the two sides of a mountain gap. Then, the pressure field at mountain-top level, where the wind is nearly geostrophic, will differ little from that at the bottom. Thus, if there is a gap in a N.-S. range of mountains, under these conditions the low pressure will be to the left of the upper wind (in the northern hemisphere) and the surface wind will blow from the west if the upper wind has a component from the north, and from the east if it has a component from the south. If the upper wind is directly across the range then the surface wind will only blow through the gap if momentum is carried down from above by mixing. In thinking about the low-level flow under these circumstances it must be realised that in the lower levels the motion is nothing like geostrophic, but is dominated by the curvature of the path and accelerations along the path of the air and by the necessity of a pressure fall through the gap.

TABLE III. Upper winds (in degrees and mi hr^{-1}) at Gibraltar, 21 to 23 July, 1944. The upper winds rotate clockwise and become reversed before the surface wind. In contrast with 17 to 18 March the cold air did not arrive as a front that could be tracked

	21 July				22 July						23 July			
Height in ft	1135 hr		1630 hr		0515 hr		1130 hr		1620 hr		2350 hr		1140 hr	
Surface	240	18	240	35	300	10	240	15	240	05	050	07	070	30
500	250	21	240	27	300	10	240	08	250	04	050	09	070	38
1,000	250	32	280	17	310	15	290	04	080	05	060	11	080	29
2,000	300	21	310	22	320	15	320	07	080	13	090	13	080	19
3,000	310	15	290	07	320	11	350	10	020	06	090	13	(into cloud	
4,000	310	19	—		—		020	07	010	07	090	13	at 1,500 ft)	
5,000	260	23	—		—		320	03	030	08	—			

TABLE IV. Upper winds (in degrees and mi hr^{-1}) at Gibraltar 13 to 14 Aug. 1944. Clockwise rotation of the upper wind through south followed by a change in the surface wind to westerly

	13 Aug.						14 Aug.	
Height in ft	1120 hr		1640 hr		2315 hr		0915 hr	
Surface	070	28	070	17	090	10	290	05
500	070	36	070	22	090	09	290	06
1,000	080	31	090	23	090	09	290	07
2,000	100	23	110	19	080	09	290	07
3,000	100	21	—		190	07	290	11
4,000	100	28	—		210	04	250	15
5,000	130	19	—		210	23	250	15

When the change in pressure distribution is much the same at all levels, and if he direction of the isobars is rotating so that the upper wind backs or veers steadily hen eventually the surface wind will reverse, quite suddenly, when the pressure fall hrough the gap is reversed. In Tables III and IV are seen examples of clockwise otations of the upper wind with a sudden change of the lower wind. These examples re quite typical. It may be noted that when the reversal becomes established on these wo occasions, as often, the upper wind is backed from the surface isobars because here is some effect of the arrival of surface cold air.

When the surface wind is going to reverse, the upper wind nearly always rotates n a clockwise direction. This is not primarily due to a rotation of the direction of the sobars but to an isallobaric component in the upper wind. For when the surface vind is from a cold air mass to the west the upper wind will be from the W. or SW., lthough the surface isobars indicate a NW. gradient wind, because of the thermal omponent. The upper gradient wind therefore changes from W. or SW. to E. or NE. so that there is falling pressure to the S. or SE. and rising pressure to the N. or NW. There is therefore a northerly isallobaric component in the upper wind as it hanges from westerly to easterly and it therefore rotates clockwise. Similarly when t changes from easterly to westerly it changes through south. That the reversal by otation of the upper wind was a prognostic of a reversal of the lower wind has long been known at Gibraltar.

5. Changes in stability

The reality of the importance of momentum exchange with the layers above the mountain tops is well illustrated in Table V. It gives the upper winds at Gibraltar on wo occasions when the direction of the isobars remained unchanged. During the ight and morning the wind blew through the straits from the west towards low pressure, ut in the afternoon and evening gained a northerly component of momentum from he air above through mixing. Under these circumstances the noticeable feature is ot a change in wind strength but solely one of direction.

TABLE V. (i) Upper winds (in degrees and mi hr^{-1}) at Gibraltar, 16 and 19 November 1943. The lower-level winds tend to conform to the winds above when mixing is greatest, otherwise they tend to blow towards the low-pressure end of the straits. (ii) Surface winds at North Front (sea level) showing the afternoon veer

)	16 Nov.						19 Nov.			
Height in ft	1310 hr		1650 hr		1030 hr		1650 hr		2215 hr	
Surface	290	06	320	12	280	08	310	05	330	11
500	300	09	330	15	290	06	360	04	300	11
1,000	330	08	340	17	330	06	010	06	310	13
1,500	330	09	340	18	330	11	010	10	310	13
2,000	340	10	350	15	340	13	010	11	310	15
2,500	360	08	320	19	350	15	010	11	320	15
3,000	010	10	330	16	350	15	360	11	330	18
4,000	360	18	350	24	350	16	340	11	350	17
5,000	010	17	340	11	350	17	340	12	340	14

The last set of wind readings on 19 Nov. is at a height of 4,500 ft.

i)

GMT)	0400 hr	0700 hr	1000 hr	1300 hr	1600 hr	1800 hr	2200 hr
5 Nov.	W. 20	WNW. 18	W'N. 10	NW. 5	NNW. 6	NNW. 4	NW. 10
9 Nov.	W. 5	WNW. 3	W. 3	NW. 6	NNW. 6	NW. 3	NW. 2

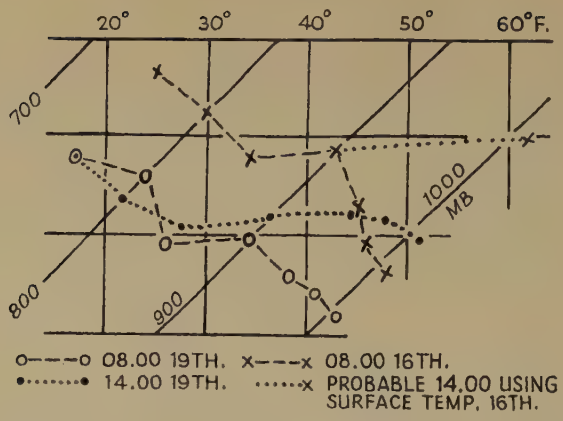

Figure 10. Upper-air temperatures, Gibraltar, 16 and 19 Nov. 1943. See Table V.

However, changes in stability may produce changes in speed only. A remarkable feature of the easterly winds at Gibraltar blowing from a cold pool of air in the Mediterranean is that they often increase at the surface at night. This happens when the cold air is in more or less neutral equilibrium, and is capped by a very stable, though often thin, layer. At night the stability of the lowest layers is increased by radiation and katabatic winds. If the wind decreases with height the surface air will therefore lose less momentum upwards than before and the surface wind will increase at night. This is very common, and is remarkable to observe. If the wind increases with height the surface wind will decrease at night for it will gain less momentum from above than during the day ; this happens about as often but surprises no one. The increase or decrease of wind speed with height referred to is that within the cold air. Anything may be taking place above the stable layer. These changes take place in the surface wind with no observed (as yet) accompanying change in the pressure field.

6. 'Capricious' wind changes

In many places the direction of light winds is of no concern. At Gibraltar a light wind may help an aircraft to land safely on a runway which has sea at both ends, it can determine the drift of sea-fog patches which may obscure the runway for hours or if the bay is full of ships it may determine the drift of heavy clouds of smoke. *A* wind of 2 mi hr^{-1} may be all-important.

Katabatic winds at Gibraltar are from the W. or NW., and are fog-free, but being shallow may only lift fog that has blown in from the sea a few feet off the airfield. Their operation may be delayed by low cloud until late in the night if the wind is light easterly, but the surface wind is eventually reversed to westerly. After dawn the wind reverts to easterly, but the upper wind may be from the SW. (low pressure then being at the western end of the straits), and as convection increases the surface wind may return to westerly point. Such ding-dong changes can be most alarming and may be influenced by the normal semidiurnal pressure wave which moves with the sun. The sharp morning-and-evening pressure rises take place earlier to the east and so in the absence of other influences may cause a light easterly wind for a short period. This seems the only explanation sometimes but obviously a very thorough investigation would be required to establish it.

7. The strength of mountain gap winds

In regions where the air below mountain-top level is characterized by great stability is common to find strong winds in the lower layers when shallow masses of cold find their way through mountain gaps and along precipitous coastlines. The editerranean is well known for the occurrence of *contrastes* or sudden changes of nd from place to place. The Mistral is one of many strong winds that manifest e penetration of cold air into the Mediterranean from the north. Another phenomenon which the term *contrastes* is applied is a change in wind direction with height apparent om the movement of clouds at different levels. If a depression approaches the Straits Gibraltar from the west, an easterly surface wind sets in as soon as the pressure lls to the west, and persists often until the low centre has entered the Mediterranean. he upper cloud moves from the SW. or W. and this contrast in direction is regarded cally as a warning of the approach of weather characteristic of a depression.

The high speeds achieved by ravine winds has been described by Hare (1941), ut high speeds can also be attained in larger mountain gaps. Unfortunately pilot-alloon ascents are unable to reveal the lower-level winds at Gibraltar when they are rong on account of the eddies, but Catalina flying boats have frequently been able to main almost motionless and at times fly backwards in the Straits of Gibraltar in rong easterly winds whose speed above the mountain tops was of the order of 20-30 kt. t heights of 100 to 400 ft speeds of 100 kt can occur but they are very localized, and o not concern aircraft which normally fly in the region at heights of 2,000 ft or more. he advice given to pilots wishing to fly through the Straits against strong winds is fly above, not below, mountain-top level.

8. Application of the ideas

The principles outlined above can be applied to any valley or mountain gap, nd are most useful when the air is very stable in the lower layers or has a very stable yer below the mountain tops.

For instance, the Bann valley, extending from Lough Neagh in N. Ireland north-ards to the sea is enclosed by hills which reach 800 ft along the entire length of the lley, while the river descends only 50 ft in 30 miles. On the night of 26 Jan. 1945, hen the lowest layers were very much cooled by radiation, a northerly surface wind eveloped in the valley in a south-easterly gradient. Again on the similar night of 0 Jan. 1945, the surface wind became southerly at night after blowing from the west ith the upper wind all day and having fallen calm after dusk. The direction of the obars was unchanged. This is more than the ordinary backing of the wind at night, r the change in direction may amount to as much as 120° as the air stabilises and the rface wind blows towards the low-pressure end of the valley. Light winds such as ese can be crucial in determining the location of patches of fog on radiation nights.

Acknowledgment

This paper is published with the permission of the Director of the Meteorological ffice, and is based on notes made during 1943-5.

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551.513.1 : 551.55

Note on the seasonal variation of the meridional flux of angular momentum

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(Manuscript received 19 June 1951)

SUMMARY

A marked seasonal variation of the poleward angular-momentum flux for the northern hemisphere in the vicinity of 30°N. latitude is obtained from an analysis of upper-wind soundings for a period of one year. The angular-momentum flux appears to be about twice as great during the winter as during the summer. Indications of a mean meridional circulation of the strength and character necessary to provide a substantial flux of angular momentum are lacking. It is suggested that the horizontal eddy flux of angular momentum across 30°N. latitude is sufficient to account for the drain of this momentum in middle latitudes.

1. INTRODUCTION

In view of the fact that the prevailing westerlies in the middle latitudes of the northern hemisphere undergo a pronounced variation in strength from winter to summer at low levels, it follows that the rate of removal of angular momentum through surface torques within this belt also undergoes seasonal variations. Since the atmosphere does not store sufficiently large amounts of angular momentum, these variations must be reflected in corresponding fluctuations in the rate at which angular momentum is fed into this zonal belt, primarily across the southern boundary of the surface westerlies in the general vicinity of 30°N. latitude. One may then inquire whether these variations in the flux of angular momentum are revealed by actual measurements from upper-wind studies. Successful investigation of this question might serve two purposes (among other possible uses). In the first place, added information concerning the large-scale processes in the atmosphere might be obtained. In the second place, a reasonable result of such an endeavour would lend greater support for the validity of the techniques used to measure the flux involved. The aim of this note is to describe the results of an attempt to secure measurements of the type discussed.

2. PROCEDURE AND RESULTS

On a previous occasion the present writers (Starr and White 1951) reported the results of an endeavour to measure the flux of angular momentum across the latitude circle in the vicinity of 30°N. latitude from upper-wind data for the six-month period from February to July 1949, using a network of 16 observation points encircling the hemisphere. The present study is based upon an extension of these computations so as to cover a total period of one year, *i.e.*, February 1949 to January 1950 inclusive.

In order to avoid repetition, only a brief statement will be made here of the methods used to measure the angular-momentum flux, the reader being referred to the discussion in the paper already referred to, which is henceforth designated as (A). The necessary notation used may be summarized as follows :

u eastward component of the wind.
v northward component of the wind.
$[\alpha]$ space average of a quantity α over the length of the complete latitude circle.
α' deviation of a quantity α from its space average $[\alpha]$.
$\overline{\alpha}$ time average of a quantity α. Time and space averaging processes are not necessarily commutative.
$[\alpha]'$ deviation of the space average $[\alpha]$ from the space-time average $\overline{[\alpha]}$.
$\{\alpha\}$ arithmetic mean of all individual observations of α at a given level during entire time period considered.
n number of days for which observations were available at a given level.
N number of individual observations present at a given level during entire time-period considered.
r coefficient of linear correlation for the N pairs of u and v.

In (A) it was shown that the quantity $\overline{[uv]}$ which represents the northward transport of linear momentum per unit mass at a given level may be resolved into three components, namely :

(i) Transport of the first species, $\overline{[u]}\,\overline{[v]}$. This component represents the contribution due to a net meridional mass flux $\overline{[v]}$ during the entire period at the level in question. To the extent that a non-zero value of $\overline{[v]}$ at one level is usually compensated by values of $\overline{[v]}$ having an opposite sign at other levels, this component depends upon the existence of so-called *mean meridional circulations.*

(ii) Transport of the second species, $\overline{[u]'[v]'}$. This component represents the contribution from fluctuations in $[v]$ due to its possible correlation with $[u]$ in time.

(iii) Transport of the third species, $\overline{[u'v']}$. This component represents the contribution due to a correlation between u and v along the length of the latitude circle. It does not require the presence of net air motions $[v]$ or $\overline{[v]}$.

Since one basic question involved in the subject relates to the role of mean meridional circulations, it is useful to form the quantity $\{uv\} - \{u\}\{v\}$ which is analogous to the sum of the transports of the second and third species although the mode of averaging used is slightly different as explained in (A).

Because of slight changes in the tabulations of the source material and for other reasons some changes were made in the computation procedures for the second six-month period following the period used in (A). These are :

(a) For the entire year it was found feasible to add material for the 55,000 ft level, since a fairly large sample of winds could then be subjected to analysis.

(b) For the 6, 10, 20, 30, 40, and 55 thousand-feet levels supplementary data as reported for the 850, 700, 500, 300, 200, and 100 millibar levels, respectively, were added, although preference was given to constant-height data where available.

(c) Three key stations were added to the network, bringing the total number to 19. These are given in Table I.

TABLE I. LIST OF KEY STATIONS ADDED

Station	Latitude (N.)	Longitude (E.)	Altitude (ft)	Type
Farouk	30° 08′	31° 24′	223	radio wind
Habbaniya	33° 22′	43° 34′	144	,,
Hong Kong	22° 18′	114° 10′	—	pilot balloon

Since a large amount of observational material is needed in order to achieve satisfactory sampling, the data were separated only into summer and winter seasons. Thus the six months May to October 1949 inclusive were considered as representative of summer conditions, while the six months February, March, April, November and December 1949 together with January 1950 were considered as forming a composite winter season. The results are given in Tables II and III which are identical in form to Table III of (A). These tables give the transports of each of the three species in columns 6, 7, and 8 together with various other quantities defined in terms of the symbols already recapitulated here, and explained more fully in (A). It might be added that a number of the quantities carry confidence limits which are defined as twice the standard error.

The means for the entire year are in themselves of considerable interest. These are given in Table IV which is similar to Tables II and III save for the inclusion of the 55,000 ft level. As might be expected, the yearly values tend to approximate averages of the summer and winter figures, although it is interesting to observe other features such as the behaviour of the confidence limits.

In all three tables vertical integrals of several quantities with respect to pressure are given at the foot. It is apparent, as was the case in (A), that the largest transports are those of the third species. This is true for the integrals as well as for the values at individual levels. The three curves in Fig. 1 show the vertical profiles of this component for summer, winter and for the entire year. A breakdown of the number of observations available for the entire year by station and level is given in Table V. The percentages listed include reports from the key stations together with reports from alternative stations used in place of the key stations as explained in (A). It is to be noted that Table V differs in form from Table II in (A)*.

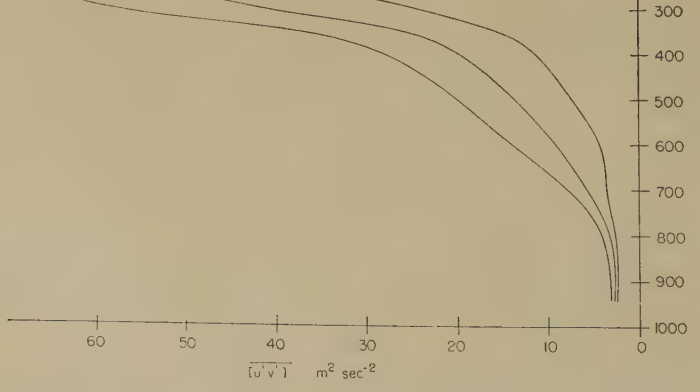

Figure 1. Vertical profiles of the eddy transport of momentum on a linear pressure scale showing the seasonal variations.

* The percentages listed in Table V are not identical in meaning to those listed in Table II of (A). Table V gives the percentage of total possible observations by station at each level, the total possible number of observations being 365 which is the number of days in a year. Table II of (A) gives percentages of actually received observations from all stations at each level as listed in Column 15 of Table III of (A).

TABLE II. Numerical analysis of data for summer. The levels are given in thousands of ft. All velocities are in m sec^{-1}. Internal consistency of the figures given is limited by rounding-off approximations.

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Level	$\overline{[u]}$	$\overline{[v]}$	$\overline{[u]}\,\overline{[v]}$	$\overline{[uv]}$	$\overline{[u]}\,\overline{[v]}$	$\overline{[u]'[v]'}$	$\overline{[u'v']}$	n	$\{u\}$	$\{v\}$	$\{uv\}$	$\{uv\}-\{u\}\{v\}$	r	N
50	+ 7·6	− 0·92 ± 0·71	− 11 ± 9	+ 3 ± 18	− 7	− 4	+ 13 ± 14	176	+ 7·5	− 0·87	+ 5	+ 11	+ 0·08	776
45	+ 11·5	− 0·47 ± 0·78	− 8 ± 12	+ 29 ± 23	− 5	− 3	+ 37 ± 16	182	+ 11·4	− 0·52	+ 34	+ 40	+ 0·22	971
40	+ 11·3	+ 0·29 ± 0·63	0 ± 9	+ 43 ± 17	+ 3	− 3	+ 43 ± 12	184	+ 11·3	+ 0·41	+ 45	+ 40	+ 0·21	1404
35	+ 9·7	− 0·39 ± 0·64	− 5 ± 8	+ 35 ± 13	− 4	− 1	+ 40 ± 10	184	+ 9·6	− 0·33	+ 34	+ 40	+ 0·25	1339
30	+ 8·1	+ 0·25 ± 0·40	+ 1 ± 4	+ 26 ± 8	+ 2	− 1	+ 25 ± 6	184	+ 8·1	+ 0·11	+ 27	+ 26	+ 0·22	1975
25	+ 6·0	− 0·02 ± 0·30	− 1 ± 2	+ 12 ± 5	0	0	+ 13 ± 4	184	+ 6·0	+ 0·03	+ 13	+ 12	+ 0·16	1918
20	+ 4·5	+ 0·15 ± 0·28	0 ± 2	+ 9 ± 4	+ 1	− 1	+ 9 ± 3	184	+ 4·5	+ 0·07	+ 9	+ 9	+ 0·15	2371
14	+ 2·8	− 0·11 ± 0·24	− 1 ± 1	+ 3 ± 3	0	− 1	+ 4 ± 2	184	+ 2·7	− 0·12	+ 4	+ 4	+ 0·09	1974
10	+ 1·6	+ 0·01 ± 0·18	0 ± 0	+ 4 ± 2	0	0	+ 4 ± 2	184	+ 1·6	+ 0·02	+ 3	+ 3	+ 0·09	2638
6	− 0·4	+ 0·18 ± 0·19	0 ± 0	+ 3 ± 1	0	0	+ 3 ± 1	184	− 0·4	+ 0·12	+ 3	+ 3	+ 0·10	2355
2	− 0·8	− 0·35 ± 0·16	+ 1 ± 0	+ 3 ± 1	0	0	+ 2 ± 1	184	− 0·8	− 0·33	+ 3	+ 3	+ 0·11	2393
Integral (10^7 cgs units)				+ 10·3	− 0·2	− 0·6	+ 11·0				+ 10·6	+ 10·9	Sum	20,114

TABLE III. NUMERICAL ANALYSIS OF DATA FOR COMPOSITE WINTER. THE LEVELS ARE GIVEN IN THOUSANDS OF FT. ALL VELOCITIES ARE IN M SEC^{-1}. INTERNAL CONSISTENCY OF THE FIGURES GIVEN IS LIMITED BY ROUNDING-OFF APPROXIMATIONS.

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Level	$\overline{[u]}$	$\overline{[v]}$	$\overline{[u]}\,\overline{[v]}$	$\overline{[uv]}$	$\overline{[u]}\,\overline{[v]}$	$\overline{[u]'[v]'}$	$\overline{[u'v']}$	n	$\{u\}$	$\{v\}$	$\{uv\}$	$\{uv\}-\{u\}\{v\}$	r	N
50	+ 18·2	− 0·63 ± 0·99	0 ± 25	+ 41 ± 38	− 11	+ 12	+ 41 ± 20	127	+ 17·7	− 0·59	+ 42	+ 53	+ 0·34	381
45	+ 22·2	− 0·84 ± 1·16	− 2 ± 32	+ 51 ± 46	− 19	+ 16	+ 53 ± 23	149	+ 21·9	− 0·51	+ 59	+ 70	+ 0·24	546
40	+ 23·7	− 0·13 ± 0·97	+ 5 ± 27	+ 62 ± 34	− 3	+ 8	+ 57 ± 18	178	+ 23·3	+ 0·39	+ 59	+ 69	+ 0·26	1046
35	+ 21·2	− 0·25 ± 0·87	+ 3 ± 21	+ 67 ± 30	− 5	+ 9	+ 63 ± 23	180	+ 21·1	− 0·20	+ 71	+ 75	+ 0·26	1030
30	+ 19·8	+ 0·10 ± 0·61	+ 2 ± 14	+ 58 ± 19	+ 2	+ 1	+ 56 ± 13	181	+ 19·2	+ 0·17	+ 62	+ 58	+ 0·25	1748
25	+ 14·8	− 0·08 ± 0·51	− 1 ± 8	+ 31 ± 12	− 1	0	+ 31 ± 9	181	+ 14·9	+ 0·02	+ 33	+ 33	+ 0·20	1728
20	+ 12·6	+ 0·15 ± 0·36	+ 1 ± 5	+ 23 ± 8	+ 2	− 1	+ 22 ± 5	181	+ 12·1	+ 0·13	+ 22	+ 20	+ 0·16	2259
14	+ 8·3	+ 0·51 ± 0·31	+ 6 ± 3	+ 20 ± 5	+ 4	+ 2	+ 14 ± 4	181	+ 8·2	+ 0·42	+ 19	+ 16	+ 0·17	1795
10	+ 5·1	+ 0·20 ± 0·21	+ 1 ± 1	+ 9 ± 3	+ 1	0	+ 8 ± 3	181	+ 5·1	+ 0·17	+ 9	+ 8	+ 0·13	2485
6	+ 2·6	+ 0·70 ± 0·36	+ 2 ± 1	+ 6 ± 2	+ 2	0	+ 4 ± 2	181	+ 2·6	+ 0·47	+ 6	+ 5	+ 0·10	2205
2	− 0·7	− 0·18 ± 0·25	+ 1 ± 1	+ 4 ± 2	0	0	+ 3 ± 2	181	− 0·6	− 0·17	+ 4	+ 4	+ 0·10	2136
Integral (10^7 cgs units)				+ 22·8	− 0·2	+ 1·9	+ 21·1				+ 23·3	+ 23·5	Sum	17,359

TABLE IV. Numerical analysis of data for entire year. The levels are given in thousands of ft. All velocities are in m sec^{-1}. Internal consistency of the figures given is limited by rounding-off approximations.

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Level	$\overline{[u]}$	$\overline{[v]}$	$\overline{[u]}\,\overline{[v]}$	$\overline{[uv]}$	$\overline{[u]}\,\overline{[v]}$	$\overline{[u]'\,[v]'}$	$\overline{[u'v']}$	n	$\{u\}$	$\{v\}$	$\{uv\}$	$\{uv\}-\{u\}\{v\}$	r	N
55	+ 8·3	− 0·17 ± 0·59	− 1 ± 10	+ 12 ± 12	− 1	0	+ 14 ± 7	255	+ 7·6	− 0·26	+ 13	+ 15	+ 0·15	834
50	+ 12·0	− 0·80 ± 0·58	− 6 ± 12	+ 19 ± 19	− 10	+ 4	+ 25 ± 12	303	+ 10·9	− 0·78	+ 17	+ 26	+ 0·17	1157
45	+ 16·3	− 0·63 ± 0·68	− 6 ± 16	+ 39 ± 24	− 10	+ 5	+ 44 ± 13	331	+ 15·6	− 0·51	+ 38	+ 51	+ 0·22	1517
40	+ 17·4	+ 0·08 ± 0·58	+ 2 ± 14	+ 52 ± 19	+ 1	+ 1	+ 50 ± 11	362	+ 16·4	+ 0·39	+ 39	+ 52	+ 0·22	2450
35	+ 15·4	− 0·32 ± 0·53	− 1 ± 11	+ 51 ± 17	− 5	+ 4	+ 52 ± 12	364	+ 14·6	− 0·28	+ 51	+ 55	+ 0·24	2369
30	+ 13·9	+ 0·17 ± 0·36	+ 2 ± 7	+ 42 ± 10	+ 2	− 1	+ 40 ± 7	365	+ 13·3	+ 0·14	+ 43	+ 41	+ 0·23	3717
25	+ 10·3	− 0·05 ± 0·29	− 1 ± 4	+ 21 ± 6	− 1	0	+ 22 ± 5	365	+ 10·2	+ 0·02	+ 21	+ 21	+ 0·17	3646
20	+ 8·5	+ 0·15 ± 0·22	+ 1 ± 3	+ 16 ± 4	+ 1	− 1	+ 15 ± 3	365	+ 8·2	+ 0·10	+ 15	+ 14	+ 0·15	4630
14	+ 5·5	+ 0·20 ± 0·20	+ 2 ± 2	+ 12 ± 3	+ 1	+ 1	+ 9 ± 3	365	+ 5·3	+ 0·14	+ 11	+ 10	+ 0·15	3769
10	+ 3·3	+ 0·10 ± 0·14	+ 1 ± 1	+ 6 ± 2	0	0	+ 6 ± 2	365	+ 3·3	+ 0·09	+ 6	+ 5	+ 0·11	5123
6	+ 1·1	+ 0·43 ± 0·20	+ 1 ± 0	+ 4 ± 1	0	+ 1	+ 3 ± 1	365	+ 1·1	+ 0·29	+ 4	+ 4	+ 0·10	4560
2	− 0·7	− 0·27 ± 0·15	+ 1 ± 0	+ 3 ± 1	0	0	+ 3 ± 1	365	− 0·7	− 0·25	+ 3	+ 3	+ 0·10	4529
Integral (10^7 cgs units)				+ 16·8	− 0·4	+ 0·7	+ 16·5				+ 16·9	+ 17·8	Sum	38,301

TABLE V. PERCENTAGE OF TOTAL POSSIBLE OBSERVATIONS AT EACH LEVEL FOR EACH STATION FOR YEAR

	Elevation in thousands of ft											
Station	2	6	10	14	20	25	30	35	40	45	50	55
Qrendi	36	53	55	30	46	25	50	6	26	4	8	0
Bahrein	25	56	57	21	53	18	50	2	40	1	13	14
Hyderabad	99	0	98	0	87	63	37	1	0	0	0	0
Dibrugarh	98	0	87	0	52	36	24	0	0	0	0	0
Tokyo	93	93	94	85	93	88	87	73	59	45	31	19
Midway	79	79	72	63	62	49	47	39	35	25	20	15
Honolulu	94	94	94	90	91	79	81	69	63	47	39	37
Weather Ship (a)	86	82	83	79	76	65	59	44	40	28	21	17
Santa Maria	99	99	99	99	97	93	86	79	73	59	46	35
Big Spring	—	99	99	99	93	87	77	66	60	48	35	22
New Orleans	99	98	95	93	90	80	72	64	54	41	25	9
Miami	100	99	99	99	89	93	89	83	75	58	42	24
Kindley Field	83	82	76	58	56	39	30	16	10	4	2	2
Weather Ship (b)	73	85	85	82	82	76	72	64	58	39	32	23
Lagens	78	81	79	74	75	65	62	44	37	15	6	7
North Front	51	56	55	53	50	46	53	6	18	5	12	5
Habbaniya	7	35	32	1	30	1	28	0	25	0	0	16
Hong Kong	36	23	16	7	8	1	5	0	2	0	0	0
Farouk	4	33	32	1	28	0	14	0	4	0	0	0

3. Discussion

Several points of importance may be noted concerning the general results. Among these are the following :

(a) The transports of the first species are small except possibly at high levels during winter. In general the impression is given that no well-defined seasonal variation exists in the figures for individual levels and none exists in the vertical integrals.

(b) The transports of the second species are also small except possibly at high levels during winter. None of the values is positive during summer, while with one minor exception at 20,000 ft none of the values is negative during winter. The integrals, of course, change in the same sense during the year. It may therefore be that this component possesses a seasonal variation although the magnitudes involved are small.

(c) The transports of the third species are by far the most important. They are much larger during the winter at every level, making the integral for this season about twice as large as the integral for summer. These results are also apparent from Fig. 1. The maximum transport (per unit isobaric layer) occurs at 35,000 ft during winter and at 40,000 ft during summer.

(d) In general the quantities $\{uv\} - \{u\}\{v\}$ in columns 13 agree quite well with the sum of the items in columns 7 and 8, as is also true of the corresponding integrals. This shows that somewhat different methods of handling the data yield comparable results.

(e) The confidence limits for the transports of the third species in columns 8 exclude zero in all instances as was the case in (A), except at 50,000 ft during summer.

(f) The quantities $[\overline{v}]$ and $\{v\}$ are again generally small in all three tables. The exact values cannot be taken too literally in view of the confidence limits

obtained and for other reasons discussed in (A). However, it should again be stressed that in studies of the present kind large values for these quantities would have indicated improper sampling of the hemisphere with respect to troughs and ridges in the streamline patterns, as the first possibility.

Because of the well-known lack of isotropy of the eddy processes of the general circulation from longitude to longitude, this problem of securing an adequate sampling of the hemisphere cannot be over-emphasized. Even more especially it is the *bête noire* in any attempt to secure measurements of the net meridional flow of air at given levels in the atmosphere.

As discussed in (A) there is a tendency for larger positive values of $[v]$ and $\{v\}$ to occur at lower levels during winter. The appreciable negative values obtained at high levels are interesting but selectivity and less satisfactory sampling at these levels doubtless are important factors to be considered.

Over long periods of time the net mass flux across any latitude circle must vanish. In view of this it should be expected that the vertical integrals of $\overline{[v]}$ and $\{v\}$ with respect to pressure should be small. Such integrations were performed and this condition is fulfilled.

(g) Estimates of the surface frictional torques made by Priestley (1951) reveal a required yearly mean flux of about $31{\cdot}7 \times 10^{25}$ CGS units of angular momentum/sec across latitude 30°N. In the present study the yearly mean eddy flux of angular momentum in the same units is $30{\cdot}9 \times 10^{25}$, in good agreement with Priestley's estimated requirement. Both of these quantities may however be underestimates rather than overestimates.

References

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551.508.25, 551.521.16

Photoelectric measurements of the seasonal variations in daylight at Plymouth from 1947 to 1949

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Summary

Daylight received on a horizontal surface has been measured for fourteen years using a Burt vacuum sodium cell and Cambridge thread-recorder galvanometer. Mean monthly values of daily maxima, in kilolux, are tabulated, also mean monthly values of illumination in kilolux hours. These are compared with similar measurements at Kew in 1947 and 1948, obtained using a selenium cell. The kilolux hours for each month are shown as a percentage of individual years, with mean values for fourteen years and percentage range for variation. Annual totals were 108, 124 and 122 megalux hours for 1947, 1948 and 1949 respectively with 116 as the mean of the whole series. Between these and sunshine or rainfall there is no simple relation. Cloudless days are not the brightest owing to downward reflection from clouds. Darkest days in each month commonly show 17 to 40 per cent as many kilolux hours as do brightest, though smoke pollution may reduce illumination to 5 per cent.

1. Introduction

The objects of this paper are to present the observations made since 1947 and to compare them with those already published ; also to consider the observations on illumination in relation to those ordinarily recorded by the Meteorological Office for sunshine, rainfall and cloud, especially the last.

2. The photoelectric measurement of daylight

The use of a Burt vacuum sodium cell and Cambridge Instrument Co. ' thread recorder ' has already been described (Atkins and Poole 1930, 1936) and an account has been given of the standardization of the cells (Poole and Atkins 1935). The 1936 paper, also Atkins (1938) and Atkins & Ellison (1947) contain the results obtained up to March 1941 when war damage temporarily ended the work. The measurements relate to the ultra-violet, violet and blue, but chiefly to the region about 0·41 μ, at which the sodium cell exhibits a sharp maximum of sensitivity in a mean noon-sunlight spectrum. The Burt cell prepared by electrolysis of the glass was chosen for the purity of its sodium, for its constancy and for its sensitivity, as it was required to give a full-scale deflexion close to 5 μa in mid-winter daylight so as to make best use of the recorder chart.

The cell used with the recorder since December 1929 has had, in all, 15 years' exposure and had previously been used for submarine photometry (Poole and Atkins 1931). Experience with other cells of the same type in England, Ceylon (Atkins, Ball & Poole 1937) and the Sudan confirms their permanence, provided they are mounted so as not to get hot enough to cause evaporation of the sodium. The latter condenses on the window and elsewhere, so producing a yellowish (minus blue) colour filter and a conducting layer which shorts the electrodes and gives a dark current from the high-

ension batteries. The Plymouth cell suffered some such damage in a fire on the night f 20 Mar. 1941. The cell was, however, usable and by daylight comparison with a imilar cell (standardized against an electric arc and reserved for such comparisons) t was found to have 78·5 per cent of its original sensitivity. Comparison of the maxi- num illumination for a very bright day in March 1947 with the maximum for the ame day in six other years (Atkins & Ellison 1947, Table I) gave 79·8 per cent. The nean gives a sensitivity of 4·8 klx/μa, as against 3·8 formerly. Unfortunately in a ale of 30 Jan. 1948 the diffusing opal was blown away. Fresh comparisons were nade against the relatively unexposed cell in sunshine and clear blue sky, because he transmission of the new diffusing opal differed from that of the old. Thirteen leterminations under very steady conditions gave as a mean a sensitivity of 7·2 klx/μa. Comparisons of bright days in March 1948 and 1949 with maximum values for similar lays in the 6 years already mentioned gave 6·90 klx/μa for the mean of six. The veighted mean is 7·1 klx/μa.

When new the recorder cell had been found, on an optical bench, to give a recti- inear relation between light intensity and current up to 40 klx. As a result of the ire it no longer gives results in accord with the comparison cell in very clear summer veather with high-altitude sun. This arises from its loss in short-wave sensitivity ccasioned by the sodium film and because the blue, violet and ultra-violet are rela- ively greater when the sky is unwashed with white. No serious error occurs save for igh intensities and short periods, so that the areas of the curves, namely the illumi- ation in kilolux hours are but little affected. A similar comparison of the relatively nexposed arc-standardized cell with a third Burt cell, which had two years' continuous xposure in addition to occasional use, showed that this had maintained its constant atio, just as the recorder cell had done before the fire.

3. Illumination throughout the years 1947-9

In the tables of illumination which follow, heavy type denotes maxima, either or the year or, in the vertical columns, for the months. The measurements relate to Plymouth, latitude 51° 5′N. for the years 1947-9 and mean values are also given for 1930-1940 or in some cases up to and including February 1941. For comparison and hrough the courtesy of Dr. J. M. Stagg (private communication 1950) and by permission of the Director of the Meteorological Office observations for Kew are also given for 1947 and for 1948, or for a combination of observations made in those years. The Kew results were obtained with a selenium rectifier cell, which was renewed at the end of each year as its current/illumination relation was no longer rectilinear for strong ntensities. This cell is more sensitive to the middle of the spectrum than is the sodium cell ; consequently it is relatively more sensitive to sunlight than to light from a blue sky.

(*a*) *Daily maximum vertical illumination*

Daily maximum vertical illumination in kilolux is shown in Table I. Both at Plymouth and Kew, February 1947 was a dark month. In spite of the difference in the cells the maxima for the Plymouth and Kew records are often close together as in May, nd almost identical in April 1947. In June the Kew observations are definitely higher nd even exceed the high Plymouth value for May 1930. Kew stands higher than the ame years at Plymouth in summer, even higher sometimes than any Plymouth year.

TABLE I. Daily maximum vertical illumination in kilolux, at Plymouth, latitude 51°5′N. Top section, least values ; middle section, greatest values ; bottom section, monthly mean of daily maxima. The marked fifth and sixth lines of the middle section relate to observations at Kew using selenium cell

	J.	F.	M.	A	M.	J.	J.	A.	S.	O.	N.	D.
1947	10·4	4·2	20·1	39·5	29·1	30·6	41·2	42·9	34·2	10·7	4·6	3·5
1948	4·4	6·7	19·5	43·0	85·9	47·2	29·1	52·9	17·4	15·3	8·2	3·3
1949	5·3	18·2	19·7	31·6	54·7	85·2	58·2	27·7	24·1	6·7	8·7	8·1
1930-Feb. 41	3·0	4·5	16·1	28·1	35·7	24·7	22·0	19·2	19·0	8·4	3·2	1·4
1947	31·8	50·4	99·4	143	139	**141**	135	107	109	59·7	37·8	24·7
1948	39·9	60·5	103	149	**158**	154	149	116	101	63·5	40·5	28·5
1949	30·4	61·8	99·8	135	**151**	148	126	136	101	64·3	43·3	24·3
1930-Feb. 41	39·5	82·5	130	196	198	175	195	193	131	98·4	54·0	31·9
1947*	> 50	45	109	144	149	168	**172**	152	129	76	48	31
1948*	56	> 61	103	> 107	151	**211**	171	165	109	122	58	32
1947	20·8	27·3	61·1	92·4	104	**107**	100	71·1	69·8	41·6	20·5	14·2
1948	16·3	33·7	66·4	104	117	**118**	106	89·7	70·3	45·9	26·1	16·7
1949	22·0	39·3	64·2	93·0	**114**	111	96·7	83·4	64·5	45·4	25·5	17·3

The mean values for the daily maxima in the three years at Plymouth are in good agreement. This is evidence that the recalibration of the photocell after the loss of the opal has been accurate. For example April 1947 and 1949 are almost identical, June 1947 is slightly below 1949 and July slightly above, September 1947 and 1948 are almost equal.

(*b*) *Vertical illumination integrals*

Table II contains the illumination integrals. This shows that while as in Table I the effect of direct sunlight on the selenium cell may lead to an exceptionally high illumination yet both sodium and selenium cells measure diffused daylight similarly. Thus May 1948 Plymouth is followed by June 1949 ; then come June 1947, Kew and June 1949 Kew, lastly June 1947 Plymouth, though only the last is below the fourteen-year average for Plymouth in May and June, which are almost identical. Again both Plymouth and Kew had 182 klx hr/day in October 1947, but show a real difference in 1948 with 184 and 228 respectively. From June to September there is a tendency for Kew to lead, doubtless due to an excess of cloud in the wetter western district. Kew also had a brighter January 1947, well above the Plymouth average, whereas February 1947, low at Plymouth with 103 klx hr (average 138) was at Kew down to 52·2, with March only 169, against Plymouth's 241 with average 301. There is thus a clear indication of heavy smoke pollution at Kew. The yearly averages in klx hr/day are Kew 345, Plymouth 339 for 1948 ; Plymouth 335, for 1949 ; and Kew 326, Plymouth 296 for 1947 ; the Plymouth fourteen-year average being 319.

(*c*) *Percentage of yearly illumination in each month*

Table III shows the percentage of the light for the whole year which is received in each calendar month, together with winter and summer percentages, their maxima and minima and range as a percentage of the mean, with lastly the mean number of klx hr/day for each month. Thus we have a minimum mean value of 1·3 per cent for

ABLE II. Vertical illumination integral in klx hr/day, at Plymouth. The sections show ast, greatest and monthly mean values. The fourth lines of first and second sections are ean two-year values for Kew. The fourth and fifth lines in the third section relate to Kew, measurements with selenium cell

	J.	F.	M.	A.	M.	J.	J.	A.	S.	O.	N.	D.	Year
947	29·3	16·6	63·3	139	187	115	231	219	153	45·4	14·7	9·5	—
948	12·3	27·7	90·0	195	288	262	172	115	85·9	45·4	32·7	11·6	—
949	16·9	59·8	72·0	107	255	464	288	132	97·0	31·0	27·7	26·3	—
947 and 1948*	8	6	25	52	133	264	192	185	88	44	11	7	—
930-Feb. 1941													
nd 1947-1949	11·5	16·6	63·3	107	120	109	135	115	81·4	33·3	14·2	6·2	—
947	114	210	499	798	852	**873**	811	622	467	336	144	81·3	—
948	144	296	554	826	**964**	948	953	652	447	346	166	94·5	—
949	135	248	449	762	**943**	892	790	668	561	310	143	86·1	—
947 and 1948*	225	247	530	763	959	**1061**	941	869	695	515	237	102	—
930-Feb. 1941													
nd 1947-1949	186	371	717	1230	1188	1206	1323	1023	743	414	207	97·7	—
947	73·2	103	241	466	540	**572**	516	429	308	182	69·5	47·7	296
948	49·2	133	328	555	**727**	624	553	432	315	184	103	56·1	339
949	73·9	153	279	468	651	**692**	599	459	308	170	81·3	58·3	335
947*	80·4	52·2	169	495	470	**676**	626	618	393	182	97·9	44·4	326
948*	71·4	157	273	482	630	**635**	593	495	432	228	96·3	46·3	345
930-1940													
nd 1947-1949	69·4	138	301	489	**606**	602	530	443	323	186	86·4	52·7	319

ABLE III. Vertical illumination integrals for each calendar month as a percentage of the otal for the individual year with corresponding values for winter and summer ; also mean ercentage values for 1930-1940 with 1947-1949, and maximum and minimum values with year of ccurrence, together with the percentage range of variation calculated on the mean value. The last column shows the 14-year mean of the vertical illumination integral

	1947	1948	1949	14-year mean	year of max	max	year of min	min	range per cent of mean	Mean klx hr/day
an.	2·10	1·19	1·88	1·83	1933	2·50	1948	1·19	**72**	69·4
eb.	2·67	3·11	3·53	3·56	1934	3·87	1930	2·60	36	138
Iarch	6·91	8·21	7·10	7·91	1933	9·60	1930	6·47	40	301
pl.	12·93	13·43	11·54	12·20	1933	13·81	1937	10·92	24	489
Iay	15·46	**18·19**	16·59	**15·82**	1948	**18·19**	1938	13·74	28	**606**
ine	**15·86**	15·10	**17·07**	15·64	1932	17·17	1933	13·98	20	602
uly	14·78	13·84	15·27	13·92	1930	16·03	1939	12·29	27	530
ug.	12·29	10·80	11·70	11·62	1930	13·38	1932	10·42	25	443
ept.	8·53	7·63	7·59	8·42	1939	9·38	1932	6·87	29	323
Oct.	5·20	4·60	4·34	4·85*	1932	5·45	1934	4·20	26	186
Nov.	1·93	2·50	2·01	2·23*	1937	3·12	1934	1·80	59	86·4
Dec.	1·37	1·40	1·39	1·32*	1937	1·67	1934	1·11	42	52·7
Winter	20·17	21·01	20·25	21·76	1932	23·53	1930	18·53	18	—
ummer	79·83	78·99	79·75	78·24	1930	81·47	1932	76·47	6	—

* Mean of 13 years only ; 1933 data missing.

TABLE IV. ANNUAL TOTALS OF THE VERTICAL ILLUMINATION INTEGRAL, IN KLX HR, WITH THE CORRESPONDING MEAN DAILY VALUE ; MEAN DAILY SUNSHINE IN HOURS ; RAINFALL IN MM AND NUMBER OF DAYS WITH RAIN, AS RECORDED FOR PLYMOUTH HOE

	1947	1948	1949	1930-1940 and 1947-1949
klx hr Plymouth	108,219	123,900	121,620	116,486
klx hr Kew	119,047	126,215	—	—
Sunshine (hr/day) Plymouth	4·21	5·00	5·33	4·60
klx hr/day	296	339	335	319
Total rainfall (mm)	939	845	817	959
Days with rain	186	198	168	185
Ratio of daily klx hr to value for 1936, mean of 1932, 1933, 1935, 1936, 1937	0·96	1·09	1·08	1·03
Ratio daily hours of sunshine to value for 1936	0·99	1·17	1·25	1·08

December with a maximum of 15·8 for May, closely followed by June 15·6 ; the maximum varying between 14·0 for June 1933, and 18·2 for May 1948. The range of the monthly percentage is quite surprising, up to 72 per cent for January to 20 per cent for June, and a range of 18 per cent for winter down to only 6 per cent for the summer half year. Again mean klx hr/day come out at 606 for May and 602 for June, but only 52·7 for December, the ratio May/Dec. being 11·5.

(*d*) *Comparison of yearly and daily illumination with sunshine and rainfall observations*

Table IV compares the yearly values for illumination in klx hr with meteorological observations ordinarily returned, sunshine, total rainfall and days with rain. While illumination for 1948 was one per cent greater at Plymouth in 1948 than in 1949, with 5·33 hours of sunshine a day, 1949 stood highest in the fourteen years and was 6·7 per cent ahead. As found in a previous paper (Atkins and Ellison 1947) there is no close relation between sunshine and illumination, nor is there between the latter and rainfall or days with rain. One is more concerned with obscuration by clouds and haze, and with atomospheric pollution as pointed out before (loc. cit.).

(*e*) *Comparison of illumination on brightest, cloudless and dullest days*

It is therefore of interest to record in Table V the mean values of the vertical illumination for the 5 brightest days at Plymouth and at Kew for each month and to compare these with the corresponding values for the number of cloudless days in each month as shown in the line below and with the 5 dullest days. Percentages for cloudless and dullest days are calculated on the values for the 5 brightest. The Kew values are not strictly comparable with Plymouth, being on the combined results of the two years. In Plymouth the brightest days were always in May, but in June at Kew, where moreover the days were brighter in May to December. Only exceptionally were cloudless days brighter than the average for the five brightest days, and they might receive only 54 per cent of that light at Kew, or 56 (1949), 73 (1948) or 76 (1947) per cent at Plymouth, the reduction being due in part probably to calm-air pollution and in the main to reflection from clouds.

The percentages for the dullest days at Kew are much below those at Plymouth for February, March, May, November and December, and low also for January, and September - they are never higher. There is therefore distinct evidence of smoke pollution.

BLE V. Top section : vertical illumination (klx hr), mean daily totals for five brightest s in each month, Plymouth, 1947-1949, and Kew, 1947 and 1948 taken together. Middle sec- n : the percentage of the klx hr on the given number of cloudless days (c) calculated on t for the five brightest days (b). Bottom section : the percentage for the five dullest days (d) calculated similarly

	J.	F.	M.	A.	M.	J.	J.	A.	S.	O.	N.	D.	Year
7	109	195	425	702	**828**	826	714	565	432	318	109	78	
8	101	262	548	785	**904**	883	890	591	432	314	148	87	
9	164	274	408	694	**893**	843	748	651	498	281	135	82	
w	160	236	456	722	924	**982**	911	798	642	382	195	92	
B 1947	—	—	86	96	101	**104**	77	83	—	95	76	—	
. C	0	0	2	5	1	3	1	13	0	4	2	0	31
8	—	100	81	97	90	102	73	84	99	95	82	88	
. C	0	1	13	4	11	1	3	2	1	4	9	6	55
9	73	**56**	94	91	90	88	93	93	79	102	—	—	
. C	3	3	6	3	6	10	6	5	2	2	0	0	47
w	**54**	76	90	83	97	**104**	99	85	69	64	55	103	
B 1947	31	17	20	26	31	36	39	49	38	23	27	24	
8	17	17	25	35	51	36	28	38	31	21	39	25	
9	20	28	28	22	36	60	48	31	28	32	27	39	
w	13	5	8	23	20	34	30	29	24	19	9	15	

For Plymouth, where there is often some pollution, the percentage for the darkest ys ranges from 17 to 49 for 1947, 17 to 51 for 1948, and 20 to 60 for 1949, common lues being 20 to 30 per cent. This is mainly due to upward reflection of light by oud, and would suggest that the albedo of the cloud was 70 to 80 per cent. The to 30 per cent is a little too low as the brightest days owe perhaps 10 per cent of their ght to downward reflection and scattering. On the other hand this loss tends to com- nsate for extinction due to pollution, so the rough value 20 to 30 per cent may stand an approximate one for average cloud transmission.

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Sonic and advective disturbances*

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Summary

This paper describes the role of sonic disturbances in redistributing the mass of the atmosphere when heating or cooling is applied to a part of it. It is shown that the effect, for meteorological purposes, of heating is a local reduction of density and surface pressure. The pressure field thus set up causes an advective disturbance which later modifies the field by transport of mass with the wind.

It is concluded that no disturbance of meteorological significance ever travels from one place to another otherwise than by advection.

1. Introduction

The school text-books explain the sea-breeze phenomenon very simply : the air over the land is warmer by day and is therefore less dense, and so the pressure there is less and the wind blows towards low pressure. The standard text-book description of convection – a heated parcel of air being less dense and therefore rising – is equally simple, and less obviously open to objection because it is a phenomenon whose dimensions are small in the atmosphere. It is not obvious what will be the consequences of heating the lower layers of the atmosphere over a large region. What will happen to the air over the heated layers ? At what levels will pressure gradients be generated and what air motion will ensue ? Authors are not unanimous in their answers. It is the purpose of this paper to restate the conclusions of Jeffreys (1917) in the light of later work, and to explain the role, in problems of this kind, of sonic disturbances and in particular of compression-gravity waves. These waves are not observable except in the case of enormous explosions, and it is from the study of these ' big bangs ' that our knowledge of them is derived. Any heating has one property in common with explosions, it causes a local increase in volume, and since this property is the significant one for our purposes we can deduce the results of heating by regarding it as an agglomeration, in space and time, of explosions.

Mathematical discussion of the properties of sonic disturbances is deliberately avoided here since it is adequately given in the papers referred to and the present purpose is to explain one aspect of the atmosphere's behaviour as simply as possible. It will be seen that old-established explanations and forecasting practice have taken the properties of the sonic disturbances for granted – perhaps unwittingly.

The discussion will refer mainly to the effect of heating the lower layers of a region of the atmosphere, but can be readily applied to cooling or to heating at any level.

2. Sound waves ; little bangs

A little bang is an explosion which causes an expansion either by converting solids rapidly into gases or by rapid evolution of heat. An atomic bomb explosion is a good example of a little bang ; it evolves a large amount of heat almost instanta-

* This rather general essay is published with the hope that it will bring to meteorologists' notice certain conclusions heretofore only argued we believe in rather mathematical papers. The later and perhaps rather provocative conclusions may stimulate some readers to express their own views on the author's thesis. (Editor).

ously which causes an expansion of the order of 0·5 km^3. A spherical shock wave vels outwards with the speed of sound, and as it passes the air is displaced outwards d room is thereby made for the increase in volume of the air that has been heated. first the heated air is at high pressure but when the shock wave travels outwards e pressure is quickly restored to about its original value.

The *sonic* disturbance need not take the form of a shock wave. For instance, if e heat were released over a period of half a minute instead of almost instantaneously e sonic disturbance would be smoothed out and the front would have travelled a stance of 9·5 km from the source before the heating was completed. The same total tward displacement of air would occur as with the shock wave ; in our example of expansion of ½ km^3 the displacement at a distance of 10 km from the source would about 80 cm. If this displacement were accomplished in half a minute it would be arcely observable. It would be impossible to detect at much greater distances.

When the expansion has taken place the density of the heated air is less than that the environment and the hydrostatic forces cause the hot air to rise. This motion termed the *advective* disturbance, and it may be contrasted with the sonic disturbance. rst it is slow in comparison ; it makes no appreciable headway before the sonic sturbance has travelled far away. Secondly it is important meteorologically, being companied by the appearance of convection cloud and quite strong winds, while e sonic disturbance causes no weather. Finally it is a motion of a kind which tends nullify pressure gradients in the atmosphere by the bulk movements of air and which volves no exchange of energy between the atmosphere and the rest of the world ; hereas the sonic disturbance is the direct result of outside interference (a heat source) d is a compression wave which travels through the air as a medium, causing a dis-acement of the air as it passes but leaving no other trace behind it.

Little bangs have been described as little because for them the atmosphere is ep. The atomic-bomb sonic disturbance lifts the tropopause about one metre and vels up as a more or less hemispherical pulse into the stratosphere until it disappears.

3. Comparison-gravity waves ; big bangs

When a lump of solid is allowed to fall into a lake an expanding circular gravity-ve travels outwards. This wave is the mechanism whereby the level of the whole e is raised when the solid falls in. Since for very large expansions the atmosphere shallow, one would expect to find circular waves of the same kind, that is gravity ves, travelling horizontally outwards from the place where the expansion (due to ating) occurs, and raising slightly the level of the whole atmosphere. This does ppen but there is an important difference between the atmosphere and the lake. In ter, sound (compression) waves travel quite independently of gravity, because essure variations change the density scarcely at all ; also the waves are accompanied no appreciable transport of mass. The air, on the other hand, is so compressible at the pressure variations cause a redistribution of mass which requires work to be ne against gravity ; also it is possible for the passage of a pressure pulse to be companied by a net transport of mass.

Sound waves are audible waves of high frequency, and as the frequency decreases avity becomes more and more important in their propagation through air because e wavelength becomes comparable with the depth of the atmosphere*. The waves

* By the depth of the atmosphere is meant some representative depth such as the height of the 100 mb surface, below which) of the mass lies,

are then compression-gravity waves, but they still travel with about the speed of soun

Compression-gravity waves have been discussed by Taylor (1929, 1936), Peker (1939, 1948) and Scorer (1950). They are produced when an external agency sets u pressure gradients ; in the present context they result from the pressure rise in a which is heated before that air expands. The waves due to any normal troposphere phenomenon would be quite unobservable, as will be seen in a moment, but Providenc has given us two big bangs within living memory, the eruption of Krakatoa in 188 and the fall of the Great Siberian Meteorite in 1908, which set up waves of this kin so large that they could be observed.

The eruption of Krakatoa released enormous quantities of heat and steam in a few minutes, and a sonic disturbance travelled to the antipodes and back about 3 time before it was dissipated beyond observation. Disturbances of pressure amounting t a millibar or two were recorded all over the world, but they were accompanied by n detectable wind or weather phenomena. The duration of the disturbance increase as the waves became dispersed so that when observed it lasted from a few minutes t an hour or so according to the distance it had travelled. If the paroxysmal eruptio had been spread over a much longer period of time even the pressure disturbanc would have been unobservable.

The Great Siberian Meteorite, whose energy was very much less, neverthele had an affect on the atmosphere equivalent to an expansion of 1,000 km^3 (Scorer 195 occurring in less than a second of time. Even then the pressure disturbance was onl about 0·1 mb in England at a distance of 5750 km. If during the morning all the ai up to a height of 0·5 km over the British Isles were heated through 3°C the expansio would also be about 1,000 km^3. The sonic disturbance produced by this expansio would be quite unobservable, because the source would be so diffuse in space and tim compared with the meteorite, but it would nevertheless occur, and its effect woul be to displace the air outwards from the heated region, leaving the density there lowe than elsewhere. The observable result of this would be a fall in hydrostatic pressur at the ground in the heated area, and this would result in an advective disturbanc consisting of sea breezes and so on, with which meteorologists are familiar.

4. Slow distributed heating

Since a sonic disturbance travels at a speed compared with which the wind i negligible, its propagation is not affected in any important way by any advective distur bances that may already exist. Slow distributed heating may therefore be regarded a successive collections of bangs, each bang sending out its sonic disturbance, the inte grated effect being smooth. For meteorological purposes the effect of heating is simply to reduce the density in the heated region and to lower the surface hydrostatic pressure We need not worry about the conservation of mass ; the rise of pressure which com pensates for the fall over the heated region is distributed by the sonic disturbance over a vast area, ultimately the whole earth.

It is important to notice that if the heating lasts nearly all day, as in the case of a sea breeze, then the early heating will, by mid morning, have given rise to an advective disturbance ; but the existence of this advective disturbance makes no difference to the effect of the later heating. The later heating merely serves to reduce the density of the heated air, and the advective motion of the air is irrelevant to the sonic disturbance which achieves that reduction.

For example, when air is cooled by radiation a sonic disturbance causes a rise in ressure. The rise in pressure causes subsidence as part of the advective disturbance nd the adiabatic warming may result in an increase in the radiative loss of heat. This ss causes a rise in pressure which maintains the subsidence, and so on. The sonic nd advective disturbances operate continuously and simultaneously, but it is to be oted that the sonic disturbance affects the whole atmosphere while the advective isturbance is confined to the region occupied by the wind system of the high-pressure rea, and mass will therefore accumulate in that region.

Jeffreys (1917) discussed this problem for an incompressible atmosphere with a on-zero coefficient of expansion and arrived at these conclusions. He further pointed ut that if the heating is periodic the advective disturbance only affects the fringes f the heated area if the area is continental in size and the period a day ; and if the eriod is seasonal the pressure fall is only slowly nullified by advection because the ind is nearly geostrophic.

It is evident from Taylor's and other subsequent work that Jeffreys's conclusions pply in principle to the actual atmosphere even though the upper-boundary conditions re not identical.

5. The effect of the earth's rotation

Taylor showed that the Krakatoa wave travelled as if the earth's rotation made o difference to its propagation. In calculating the form taken by a pressure pulse fter it has travelled a great distance it is necessary to regard the pulse as composed f oscillations of all frequencies, each component frequency having its own velocity. n the equations it is evident that the earth's rotation cannot be neglected for trains f long-period waves, but when many different waves are used to synthesize a pulse hose total duration is only a small fraction of a day the rotation has no effect. It an only influence the motion of a particle if the particle moves in the same direction or an appreciable part of a day. Therefore for heating that lasts a day the rotation as some effect. The effect varies with latitude and with the period of the oscillations, ut since sound travels about 24,000 km in a day the effect is, as Jeffreys pointed out, nly on a continental scale, and even then is not appreciable. The only result of rotation s to distort a sonic disturbance.

Mention should be made at this point of a particular kind of compression-gravity ave that is well known and widely observed in low latitudes – the solar semi-diurnal ressure oscillation. This is described by Wilkes (1949). It has been shown by ekeris (1937), and Weekes and Wilkes (1947), that the wave crests lie along lines of ngitude and that the amplitude is large because of resonance, for this kind of wave st travels round the earth in a solar day. It is excited partly by tidal forces and partly y solar heating. Its mode of propagation is very much dependent on the earth's otation.

6. Lee waves

In recent years the properties of pure gravity waves in the atmosphere have become etter understood. Pure gravity waves are analogous to ordinary water waves, and do ot depend on the compressibility of the air for their propagation. There are two inds of pure gravity wave : the long waves in which the motion is mainly horizontal nd largely determined by the earth's rotation, and the shorter ones in which vertical notion predominates and the earth's rotation is ineffective. In practice they are

stationary, relative to the ground. They are found in the lee of mountains and are often referred to as *standing waves*, but the term *lee wave* is preferable because it describes their most important characteristic that they must lie downwind from something which disturbs the horizontal airflow. They are the result of some boundary constraint on the wind, but the energy of the wave motion is derived from the airstream or from the hydrostatic pressure field by flow across the isobars. This is in contrast with the compression-gravity waves which derive their energy from movement of the boundary or from some source external to the atmosphere. The compression-gravity waves cause any pressure disturbance that is set up by an external agency to be propagated through the air so fast that no pure gravity-wave motion is set up by it.

Significant properties of stable, pure, gravity waves (Scorer 1951c) are first that they travel relative to the air with a speed comparable with ordinary winds, secondly that the speed depends very much on the wavelength, thirdly that the airstream in which they travel must be of a special, though fairly common, kind (Scorer 1951b), and fourthly that they can normally only travel in one direction relative to the air if the airstream is suitable.

7. The advection principle in meteorology

Any motion resulting from the unlevel distribution of density alone is an advective disturbance. Dynamic developments too are advective disturbances. Some writers think of dynamic developments in their early stages as being unstable, pure, gravity waves, and such waves are advected with the airstream in which they are embedded*. Stable pure gravity waves and compression-gravity waves are therefore the only means by which a disturbance can be propagated through the air away from the energy-source region. Pure gravity waves, though travelling through the air, are always at rest relative to the ground, while compression-gravity waves produce no observable effect. Hence it may be concluded that all meteorological effects are therefore advected with the air, and none is propagated from one part of the earth to another through the air as medium. This is an important principle implicitly accepted in forecasting practice though never explicitly expounded†. (See, however, Scorer 1951c).

As an example of the operation of this principle we infer that the weather associated with fronts is confined to the frontal region. If the warm air is lifted at the fronts of a cyclone, that lifting is not propagated to the whole of the warm sector but remains close to where cold air is undercutting warm air. Thus if the isentropic surfaces are inclined by dynamic development (advective disturbance) they do not level out under gravity ; but if they are inclined by an external agent (heating) they are levelled out by the sonic disturbance at a rate which is, for practical purposes, almost infinite.

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* It has never been proved that unstable, pure, gravity waves necessarily travel with some part of the airstream, but it has been shown (Scorer, 1951a) that this is at least very probably so. No suggestion has ever been made to the contrary and the proposition is true of all particular cases so far studied.

† In correspondence (R. S. Scorer and J. G. Charney, 1951 *J. Met.*, **8**, p. 68) this principle is put forward. Charney, however, suggests that a developing unstable disturbance might change into a propagated stable one. Such an idea is not new though it has never been shown by experiment or theory how such a change could occur. Charney instances certain synoptic studies which are claimed to contain evidence of a propagated influence, but the reader is free in all these cases to disagree with the given interpretation of the observations.

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The diurnal march of vapour pressure on sea-breeze days at Athens, Greece

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Summary

This study is restricted to days with even pressure distribution, and without strong winds that would interfere with the sea breeze. The greatest frequency of such a sea breeze occurs in May, June and October, which are the months of low frequency of the Etesians.

The diurnal march of vapour pressure at Athens is clearly double in all months and the year as a whole. However, on days with uninterrupted sea breeze, mainly from May to October, three pairs of extremes characterize the variation, there being two secondary maxima and two secondary minima in the forenoon. The arrival of the moist sea breeze overcompensates the normal decrease of vapour pressure in the morning.

1. Introduction

The city of Athens, being at a relatively short distance (about 5 km) from the sea, often enjoys a sea breeze which blows almost invariably from the SW. or SSW. in all seasons. It occurs especially in the warm season when general atmospheric disturbances are lacking. The sea breeze usually begins 3 or 4 hr after sunrise, reaches its greatest strength in the afternoon, and gradually decreases until about sunset, when it entirely ceases. It is usually superimposed upon the stronger Etesians, but only those days during which the sea breeze prevailed without interruption are considered here. The majority of these days are characterized by even pressure distribution with almost complete calm before and after the sea breeze (Figs. 1 and 2 (III)).

The (1901-1940) average number of days having an uninterrupted sea breeze is 55·9 per year. The greatest frequencies are in May (7·3), June (6·8) and October (6·6), when the frequency of the Etesians is small (Carapiperis 1945). Sea breezes are few in winter (Dec. 1·5,

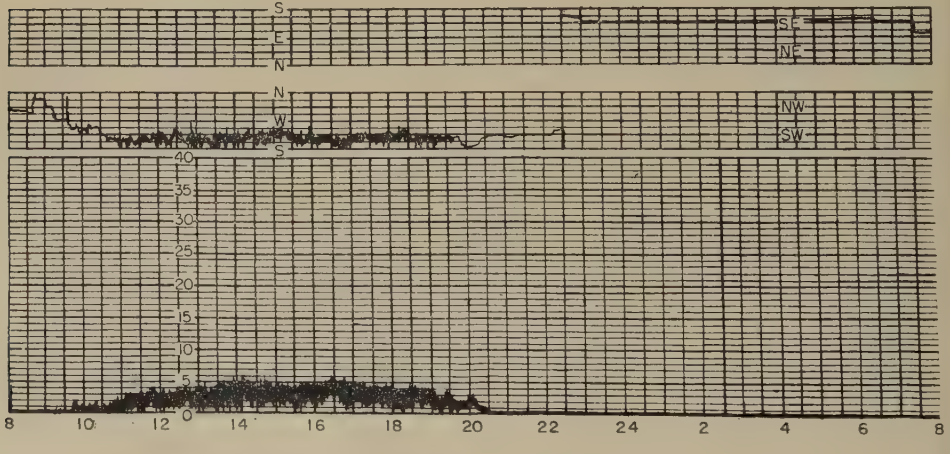

Figure 1. Typical record of summer sea breeze at Athens (7 to 8 July 1930), reproduced from Steffens-Hedde anemograph.

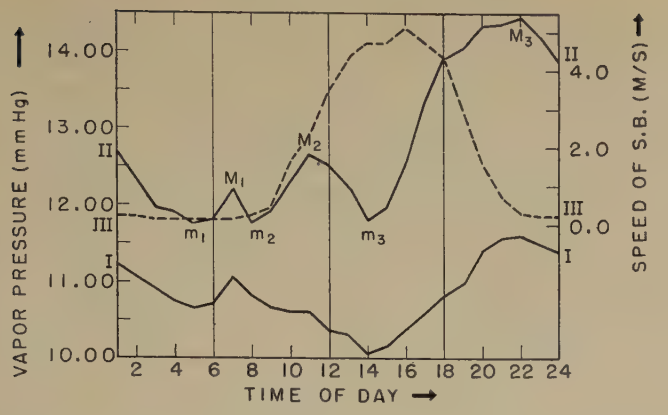

Figure 2. Average diurnal marches at Athens in July (1921-1930).

(I) Vapour pressure, all days.
(II) Vapour pressure, sea-breeze days.
(III) Wind speed, sea-breeze days.

Jan. 1·9, Feb. 2·4), and a whole month may pass without any. Such was the case in 21 per cent of the winter months.

In this relatively stable climate sea breezes may occur day after day in the warm season, even up to 9 to 13 days in succession, in May, June and October.

2. The diurnal fluctuation of vapour pressure on sea-breeze days at Athens

The diurnal variation of vapour pressure at Athens, for the period 1921-1930, shows a double wave (Karapiperis 1945). The difference between the extremes in the afternoon is much greater than that in the morning, except in winter. The afternoon minimum is the main minimum from March to October and occurs at 14 hr from May to October (Fig. 2 (I)). Therefore, the variation though having 2 maxima and 2 minima, which is one of the main characteristics of the continental type (Conrad 1936), has the main minimum in the morning only in the cooler months, November to February. The rest of the year, with a main minimum in the afternoon, is of the steppe type.

The influence of the sea breeze on the diurnal march of vapour pressure was next studied. When the marches of vapour pressure on sea-breeze days alone were averaged by months, the following features were found :

(a) Mainly in the months from May to October, the sea breeze converts the double fluctuation of the diurnal march of vapour pressure into a *triple* one (Fig. 2 (II)).
(b) The increase of vapour pressure in the late afternoon or early evening hours is great.

This type of the diurnal march of vapour pressure which may be called the *sea-breeze type*, has not been described previously to our knowledge. The triple fluctuation during winter months on the island of Jersey (Dechevrens 1905) is, obviously, not a consequence of sea breeze. Also, the diurnal triple fluctuation in the annual mean harmonic values at Rio de Janeiro (Cruls 1892) was not attributed to sea breeze by the author. Unfortunately, the actual monthly and annual values are not published. The annual values were computed by harmonic analysis from observations made every three hours (04 hr to 22 hr).

For the triple diurnal fluctuation of vapour pressure at Athens the following explanation is offered :

The first morning minimum of vapour pressure occurs, as usual in all but desert climates, at about sunrise. When the temperature increases, the evaporation increases too, and, therefore, the vapour pressure is increased for about the next two hours to the first morning maximum. When turbulent mass-exchange becomes considerable, the water vapour is distributed through a thicker layer, and therefore the vapour pressure near the ground must decrease.

This decrease of vapour pressure does not continue for more than two hours, when the second forenoon minimum is reached, because the sea breeze then sets in supplying more water vapour. The turbulent mass-exchange becomes temporarily restricted by the sea-breeze inversion, and is not strong enough to carry the water vapour near the ground rapidly to higher layers ; thus, the vapour pressure increases.

This increase continues till about 10 hr or 11 hr (to a second maximum), when the vapour pressure begins to decrease ; from this hour on, the increasing turbulent mass-exchange is more than a match for the increasing advection of vapour from the sea.

The afternoon minimum of vapour pressure occurs at about 14 hr, when evidently the mass-exchange most efficiently reduces the vapour pressure despite the advection of vapour in the sea breeze. Also, wind speed (and related turbulence) which has been increasing steadily, now comes to a temporary halt in its diurnal rise (Fig. 2 (III)).

From this hour the turbulent mass-exchange, now apparently involving little air not already somewhat humidified by the previous hours of exchange, is no longer able to dissipate the vapour as fast as it is supplied, even though the wind speed over the next two hours is somewhat greater. The sea breeze continues to transport great amounts of water vapour which, remaining largely near the ground, increase significantly the vapour pressure in the late afternoon or early evening hours. This afternoon increase of vapour pressure is especially great in the months from June to September, when the sea is warmest. The main maximum occurs between 20 hr and 22 hr.

Afterwards, with falling temperature, condensation decreases the vapour pressure until morning when, with sunrise, the temperature rises and evaporation of dew commences.

Acknowledgments

The author wishes to thank warmly Prof. C. F. Brooks and Dr. V. Conrad for their very valuable and helpful suggestions during the preparation of this paper.

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551.552 (261/4)

Vorticity of mean vector surface winds over the Atlantic Ocean

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With an Appendix by N. CARRUTHERS

(Manuscript received 30 May 1951, in revised form 24 July 1951)

1. Introduction

Vorticity in the x, y plane around the z axis is given by

$$\zeta = \frac{\partial v}{\partial x} - \frac{\partial u}{\partial y}$$

where ζ is vorticity and u, v are the components of velocity along the x, y axes, respectively. The vorticity of mean vector winds around a 10° square over the ocean is proportional to the quantity

$$\tfrac{1}{2}(E_1 + E_2 - E_3 - E_4) - \tfrac{1}{2}(N_2 + N_4 - N_1 - N_3) \sec \phi$$

where N_1, E_1, N_2, E_2 . . . are the respective northerly and easterly components in the four sub-squares and ϕ is the mid-latitude of the 10° square, as shown in Fig. 1. The vorticity refers to the central 5° square and is cyclonic if the quantity above is of positive sign and anticyclonic if it is of negative sign.

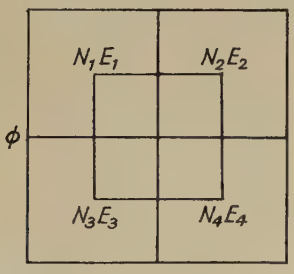

Figure 1. Calculation of vorticity of mean vector winds.

2. Vorticity of mean vector surface winds over the Atlantic Ocean

The mean vector surface winds have been computed from surface-wind observations, chiefly made by British merchant ships, during the periods 1887-99 and 1921-38 in the North Atlantic Ocean and 1855-99 and 1921-38 in the South Atlantic Ocean.

The vorticity of mean vector surface winds over the Atlantic Ocean was calculated for each month of the year for all 10° squares having at least 50 observations in each of the four 5° sub-squares. The average number of observations was of the order of 500 for each 5° sub-square. Mean values per 5° square were then computed for each 10° belt of latitude from 50°S. to 60°N. These values were used to prepare Fig. 2 which shows the mean monthly vorticity distribution as a function of latitude. The main features of the pattern are :

(a) belts of anticyclonic vorticity between 20°N. and 40°N. and between 10°S. and 40°S. in all months, associated with the semi-permanent subtropical anticyclones.

The maximum is in June in the northern hemisphere and in December and January in the southern hemisphere.

(b) a belt of cyclonic vorticity between the equator and 10°S. in all months, associated with the doldrum belt. The maximum is in July.

(c) a belt of cyclonic vorticity between the equator and 10°N. from December to May inclusive, with a maximum in February, and between 10°N. and 20°N. from June to September inclusive, with a maximum in August and September, associated with the doldrum belt.

(d) a belt of anticyclonic vorticity between the equator and 10°N. from June to November inclusive, with a maximum in August, associated with the crossing of the equator by the SE. trade winds.

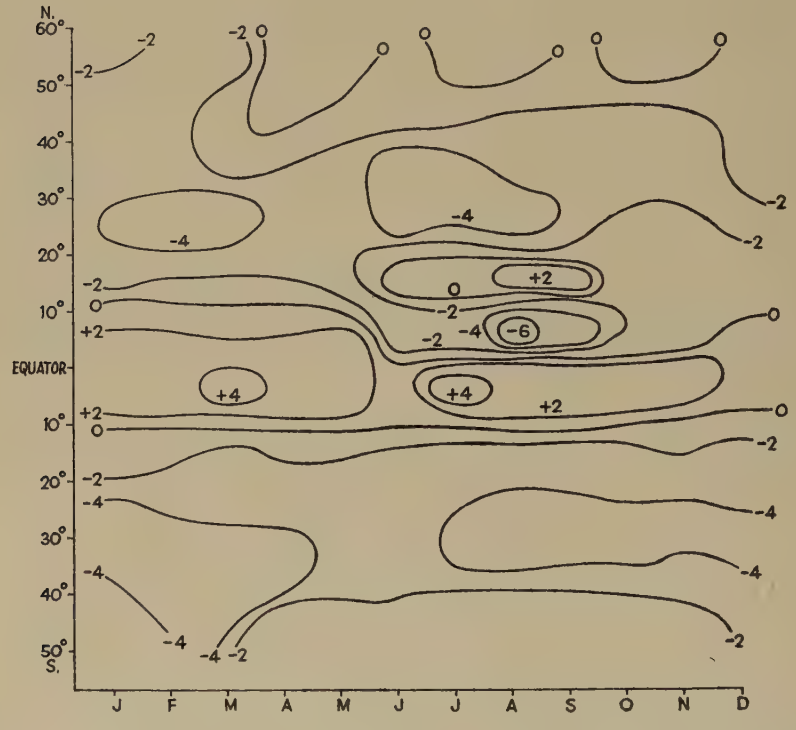

Figure 2. Pattern of mean vorticity (unit 10^{-6} sec^{-1}) over the Atlantic Ocean through the year.

Variations of vorticity with longitude are not generally significant except along the 10° to 20° belt of north latitude from June to September inclusive, during which period cyclonic vorticity is a maximum at the eastern end of the belt and decreases westwards to the Caribbean.

It is estimated that the statistical probable error of the computed vorticity values as obtained from the formula developed in the appendix varies for each belt from 10^{-7} sec^{-1} in the tropics in summer to 5×10^{-7} sec^{-1} in the temperate zone in winter.

3. Vorticity in the tropics and the formation of tropical storms

Table I gives the number of tropical hurricanes reported in the North Atlantic Ocean together with the associated mean vorticity values for the 10° to 20° belt of N. latitude.

TABLE I. ASSOCIATION OF THE NUMBER OF TROPICAL STORMS AND MEAN VORTICITY (IN UNITS OF 10^{-6} SEC^{-1}) IN 10° TO 20° LATITUDE IN THE NORTH ATLANTIC

Mean vorticity 10°-20°N.	No. of hurricanes N. Atlantic (1887-1941) Tannehill (1944)	Vorticity of square 039	No. of storms Cape Verde type (1887-1932) Tannehill (1944)
− 2·43	0	− 4·48	0
− 1·24	0	− 2·44	0
− 1·31	0	− 0·19	0
− 1·86	0	+ 0·49	0
− 2·35	0	− 1·30	0
+ 0·56	10	+ 1·77	2
+ 1·34	14	+ 6·30	11
+ 2·93	58	+ 9·67	44
+ 2·66	74	+ 7·62	64
− 0·28	39	+ 1·88	29
− 2·08	7	− 4·33	5
− 1·30	0	+ 0·85	0

There is a high degree of association between the magnitude of cyclonic vorticity in the 0° to 20° belt of N. latitude and the occurrence of tropical storms. There is no relation between the occurrence of tropical storms and cyclonic vorticity in the 0° to 10° belt of N. latitude. Tropical storms do not form in the latter belt, except perhaps in the extreme north, because the Coriolis force is too small to cause a spiral circulation to develop.

Table I also gives the number of storms believed to have *formed* in the Cape Verde region and the mean vorticity of the Marsden square 039 (= 10° to 20°N., 20° to 30°W.), in which the Cape Verde Islands are situated. This square has the highest cyclonic vorticity of the whole 10° belt of latitude from June to October inclusive. The largest number of tropical storms in the North Atlantic are believed to form in that area.

South of 10°S. the mean vorticity is anticyclonic in all months of the year up to 40°S. at least. No tropical storms occur in the South Atlantic Ocean.

ACKNOWLEDGMENT

I am indebted to the Director of the Meteorological Office for permission to publish this note.

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APPENDIX

Probable error

The probable error of any mean value is given approximately by

$$\tfrac{2}{3}(\sigma^2/k)^{1/2} \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where σ^2 is the variance of the individual values contributing to the mean value and k is their total number, provided that they are independent (see below).

The variance of a derived quantity, such as vorticity, can be expressed by elementary algebra in terms of the variances of its constituents.

For computing the vorticity of surface winds over the Atlantic, Gordon uses the simpl formula

$$\zeta = \tfrac{1}{2}(E_1 + E_2 - E_3 - E_4) - \tfrac{1}{2}(N_2 + N_4 - N_1 - N_3)\sec\phi \quad . \quad . \quad (2$$

when 5° latitude is unit distance. In this expression, let N_1, E_1 ; N_2, E_2 ; . . . now denote th components of the winds of sub-squares 1, 2, . . . at a single time of observation. Let u suppose that there are k such observation times and put $N_1 = \overline{N}_1 + n_1$, where $\overline{N}_1$ denote the mean of k values of N_1 (*i.e.*, $\overline{N}_1$ is equivalent to N_1 of Gordon's note) ; and similarly fo E_1, N_2, E_2, etc. Then the variance of ζ is

$$\sigma_\zeta^2 = \frac{1}{k}\Sigma\zeta^2$$

$$= \frac{1}{k}(\Sigma e^2 + \sec^2\phi\ \Sigma n^2 \pm \text{terms } \Sigma e_i e_j, \Sigma e_i n_j, \Sigma n_i n_j)$$

where $n^2 = \frac{1}{4}(n_1^2 + n_2^2 + n_3^2 + n_4^2)$; $e^2 = \frac{1}{4}(e_1^2 + e_2^2 + e_3^2 + e_4^2)$; and i and j denote any two of the four numerals 1, 2, 3 and 4. If we assume that the eight components, N_1, N_2, . . E_1, E_2 . . , are all uncorrelated, then the variance reduces to

$$\sigma_\zeta^2 = (\Sigma e^2)/k + \sec^2\phi\,(\Sigma n^2)/k \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Let us further assume that the k winds in each sub-square conform to a normal circular distribution so that the variances of the northern and eastern components are equal ; then if σ_i denote the standard vector deviation of winds in the ith sub-square,

$$(\Sigma e_i^2)/k = (\Sigma n_i^2)/k = \tfrac{1}{2}\sigma_i^2$$

and the variance of ζ may be written $\sigma_\zeta^2 = \frac{1}{2}\sigma^2(1 + \sec^2\phi)$ where $\sigma^2 = \frac{1}{4}(\sigma_1^2 + \sigma_2^2 + \sigma_3^2 + \sigma_4^2)$. Thus, from (1), the probable error of the mean vorticity is approximately

$$\tfrac{2}{3}\sigma\left(\frac{1 + \sec^2\phi}{2k}\right)^{1/2} \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Expression (4) holds only if successive wind observations are independent ; where there is a tendency for runs of consecutive observations to fall within the same sub-square, the value given by (4) will be too low. Observations of surface winds are believed to be independent, as a rule, only when they are two or more days apart ; hence, if successive observations within a single sub-square tend to be at 24-hr intervals, the appropriate formula for the probable error of mean vorticity is

$$\text{P.E. of } \zeta = \tfrac{2}{3}\sigma\,[(1 + \sec^2\phi)/k]^{1/2} \quad \text{for a single square ;}$$
$$\text{or P.E. of } \zeta = \tfrac{2}{3}\sigma\,[(1 + \sec^2\phi)/km]^{1/2} \quad \text{for } m \text{ squares combined} \quad . \quad . \quad (5)$$

provided that observations in each one of the m squares may be assumed independent of (*i.e.*, uncorrelated with) observations in the other $(m - 1)$ squares.

Gordon derives his limiting values of the probable error of mean vorticity from a consideration of the distribution of σ over the Atlantic, seasonal values of standard vector deviation of surface winds over the oceans having been computed some four or five years ago in connection with a Meteorological Office investigation into the variability of upper-air winds. He uses expression (5) with the substitutions $k = 500$ and $m = 4$.

551.554

Note on the derivation of the logarithmic velocity profile

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(Manuscript received 23 October 1951)

SUMMARY

In this note it is shown that under neutral conditions the logarithmic velocity profile is uniquely etermined in the fully turbulent region of the lower atmosphere.

As we may neglect the variation of mean direction with height (cf. Calder 1939) the velocity ill be given by

$$U(z) + u(x, y, z, t),\ v(x, y, z, t),\ w(x, y, z, t) \quad . \quad . \quad (1)$$

here U denotes the mean velocity in the x direction and u, v, w denote the velocity fluctuaons. As usual z denotes the height and the x and y axes are horizontal.

Let us consider a steady flow under neutral conditions over a smooth surface, *e.g.*, ice. his case corresponds to the flow of an incompressible and homogeneous fluid along an initely long plane smooth wall in the absence of external forces and mean pressure gradients. f the fluid is supposed to extend to infinity in the direction perpendicular to the wall the garithmic velocity profile can be derived without any assumption as regards the nature f the *Austausch* process.

Now consider this fluid as a dynamical system. The external influences are (i) the earing stress τ, which in the lower atmosphere can be considered constant up to a height f 30 m (cf. Ertel 1933, and Calder 1939), and (ii) the normal force exerted by the wall which, er unit area, is equal to the average air pressure. However, as only the pressure gradient ccurs in the hydrodynamical equations, the normal force cannot affect the flow under conderation. Consequently, the external influences are characterized by τ only.

The parameters characterizing the physical properties of the fluid, which are of interest r this problem, are clearly the density ρ and the viscosity μ, both assumed constant throughut the region considered.

Hence, the parameters characterizing such a flow are τ, ρ, and μ. Dimensional analysis us leads to the relation

$$U\sigma^{-\frac{1}{2}} = f\left(\frac{z\sigma^{\frac{1}{2}}}{\nu}\right) \quad . \quad . \quad . \quad . \quad . \quad (2)$$

here $\sigma^{\frac{1}{2}} = (\tau/\rho)^{\frac{1}{2}} = v^*$ denotes the friction velocity and ν is the kinematic viscosity. Here stands for an arbitrary function of $z\sigma^{\frac{1}{2}}/\nu$. All other quantities occurring in the function must be universal constants for the type of flow considered.

Introducing now the customary assumption that the molecular agitation can be neglected ove a certain height, say z_v, this implies that ν should not occur in Eq. (2) for $z > z_v$.

Let us first assume that U should be given by a power law in z. We should then have

$$U\sigma^{-\frac{1}{2}} = C\left(\frac{z\sigma^{\frac{1}{2}}}{\nu}\right)^n$$

lowever, if now U should be independent of ν this assumption implies that n must vanish. onsequently, U cannot obey a power law in the fully turbulent region.

But differentiation of Eq. (2) with respect to z yields

$$\frac{dU}{dz} = \frac{\sigma}{\nu} f'\left(\frac{z\,\sigma^{\frac{1}{2}}}{\nu}\right) \quad . \quad . \quad . \quad . \quad . \quad (3)$$

where f' stands for a new arbitrary function. Assuming now that dU/dz shall be independent of ν we obtain uniquely

$$\frac{dU}{dz} = C\frac{\sigma^{\frac{1}{2}}}{z} \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

where C denotes a constant. Integrating this equation we obtain

$$U\,\sigma^{-\frac{1}{2}} = C \log\frac{z}{a} \quad . \quad . \quad . \quad . \quad . \quad (5)$$

where C and a denote constants. Writing this equation in the form

$$U\,\sigma^{-\frac{1}{2}} = C \log\frac{z\,\sigma^{\frac{1}{2}}/\nu}{a\,\sigma^{\frac{1}{2}}/\nu} \quad . \quad . \quad . \quad . \quad (6)$$

it is seen from a comparison with Eq. (2) that C and $a\,\sigma^{\frac{1}{2}}/\nu$ must be universal constants for the flow considered.

We have thus arrived at the logarithmic velocity profile over smooth ground in the case of neutral equilibrium. Evidently, the roughness of the ground can be taken into account in the customary way by introducing a roughness parameter z_0 and a zero plane displacement (cf. Paeschke 1937).

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551.577.36 : 551.501.45

Sequences of wet and of dry days considered in relation to the logarithmic series

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Communicated by H. L. Penman
(Manuscript received 7 July 1951 ; in revised form 17 September 1951)

SUMMARY

It is shown that the frequency distribution of sequences of fine days conforms very closely to a ogarithmic ' series. The sequences of ' wet ' days do not conform so closely, as there is a shortage of e-day sequences and an excess of 2 to 4-day sequences, but this may be due to the method of defining et ' sequences.

1. INTRODUCTION

Sequences of wet or of dry days have a certain statistical characteristic to which attention s several times been drawn ; namely that the longer the spell has lasted the more likely is to last another day. The problem of the frequency distribution of sequences of different ngth has been discussed by Newnham (1916), Gold (1929), and Cochran (1938). Newnham esented some data which will be discussed below, but Gold combined the wet and the dry quences, and so probably confused the issue. He showed that the frequency distribution as not a geometric series.

In the course of many years' work on frequency distributions in biological problems, I ve had to deal with several applications of the logarithmic series, which series has the special aracter mentioned above.

The series is mathematically as follows :

$$\alpha x,\ \alpha x^2/2,\ \alpha x^3/3,\ \alpha x^4/4,\ \text{etc.}$$

here each successive term is the number of groups with 1, 2, 3, etc., units ; or, in our present oblem, the number of sequences either of wet or of fine days lasting over 1, 2, 3, etc., days. he two constants x (which is always less than unity), and α can be calculated if we know the tal number of groups and of units. Thus if in any period of time we know the number of quences of dry days (S) and the total number of dry days (N) involved, α and x can be und and the whole series calculated. This theoretical distribution can then be compared ith the observed facts.

The values of α and x can be calculated from the simultaneous equations,

$$S = \alpha \log_e (1 + N/\alpha)$$

$$\text{and } N = \alpha x/(1 - x)$$

raphical and mathematical methods of rapid solving are described in Fisher, Corbett and Villiams (1943) and in Williams (1947).

2. ANALYSIS OF RECORDS AT HARPENDEN

To test the applicability of the series an examination was made of the rainfall records Rothamsted Experimental Station, Harpenden, for two five-year periods, 1938-42, and 943-47, with the results shown in Table I. Days with ·005 in. or more of rain were onsidered as ' wet.'

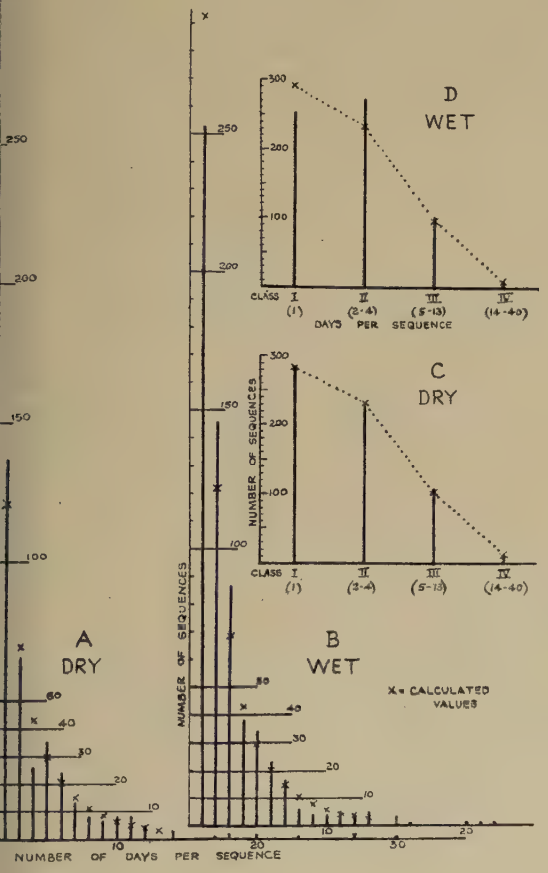
TABLE I. FREQUENCY DISTRIBUTION OF DURATION OF SEQUENCES OF WET AND OF FINE DAYS AT ROTHAMSTED EXPERIMENTAL STATION, HARPENDEN, DURING THE TEN YEARS 1938-1947, TOGETHER WITH CORRESPONDING VALUES CALCULATED FROM THE LOGARITHMIC SERIES

Length of sequence in days	DRY				WET			
	1938-1942	1943-1947	10 years Observed	Calculated	1938-1942	1943-1947	10 years Observed	Calculated
1	134	147	281 (281)	284·0 (284·0)	117	136	253 (253)	293·7 (293·7)
2	72	65	137	120·4	72	74	146	122·8
3	32	34	66	68·3	45	42	87	64·4
4	6	20	26 (229)	43·3 (232·0)	18	20	38 (271)	42·9 (230·1)
5	19	16	35	29·3	15	19	34	28·7
6	7	17	24	20·7	9	14	23	20·0
7	5	8	13	15·0	7	10	17	14·3
8	4	4	8	11·2	2	4	6	10·5
9	2	5	7	8·41	2	2	4	7·78
10	5	3	8	6·42	2	2	4	5·46
11	4	4	8	4·94	3	2	5	4·45
12	4	1	5	3·84	2	1	3	3·41
13	—	—	— (108)	3·01 (102·8)	4	1	5 (101)	2·63 (97·2)
14	2	1	3		—	—	—	
15	—	—	—		2	1	3	
16	—	—	—		1	—	1	
17	—	1	1		—	—	—	
18	1	1	2		—	—	—	
19	—	—	—		—	—	—	
20	2	—	2		—	1	1	
21	—	—	—		1	—	1	
22	—	—	—		1	—	1	
23	1	—	1		—	—	—	
27	1	1	2		—	—	—	
30	1	—	1 (12)	(11)	—	—	— (7)	(7)
Total sequences	302	328	630		303	329	632	
Total days	928	937	1865		898	893	1791	
Average days per sequence	3·07	2·86	2·96		2·96	2·71	2·83	
x	0·856	0·839	0·848		0·848	0·826	0·837	
α	155·6	179·4	334·7		160·7	188·2	348·1	

It will be noticed that the number of sequences was slightly greater in the second period 9) than in the first (303), and that the number of wet days (1791) in the ten years is 49 per t of the total. Thus the average length of the dry sequences in this locality is almost the ne as that of the wet.

At the bottom of the table are given the calculated values of x and α for each set of observa- ns. For the ten-year period the two logarithmic series frequency distributions have been culated and are given up to the 13th term. Fig. 1 (A) and (B) show the various results phically.

It is sometimes useful in comparing frequency distributions of the hollow-curve type to ke class intervals on a logarithmic scale. For this reason sub-totals are given for the observed l calculated values for the ten years for the number of sequences with 1, 2 to 4, 5 to 13 and to 40 days. These results are shown graphically in Fig. 1 (C) and (D).



ure 1. Frequency distribution of duration of sequences wet and of fine days at Rothamsted Experimental Station, Harpenden, during the ten years 1938-1947.

togram = observed values. Crosses and dotted lines = values calculated from the logarithmic series.

= dry days, B = wet days ; with arithmetic scale of duration.

= dry days, D = wet days ; with logarithmic scale of duration.

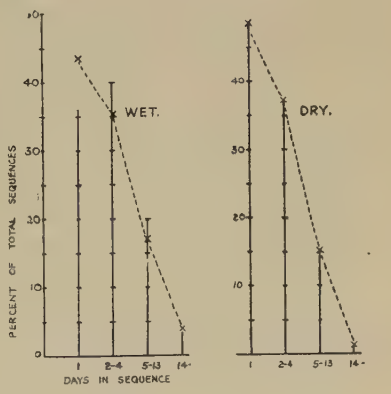


Figure 2. Frequency distribution of sequences of wet and of dry days at four localities, expressed as percentages (*i.e.*, number in 100 spells) and averaged.

Vertical lines = observed values. Crosses and dotted lines = calculated.

An examination of the results shows that in the dry sequences there is a very remarkabl agreement between the observed and calculated figures, with both arithmetic and logarithm class intervals. The observed number of single-dry-day sequences is 281 and the calculat 284 ; the observed number lasting 2 to 4 days is 229 and the calculated 232 ; for 5 to 13 da the observed is 108 and the calculated 103; while for over 13 days the figures are 12 and There is thus no doubt that the logarithmic series gives a very close representation of th frequency of sequences of dry weather of different lengths as shown by our Harpenden record

When we turn to the sequences of wet days, we find however that the observed numb of single days is well below the calculated, and the numbers with 2 days and 3 days both w above the calculated. When we use the log classes (Fig. 1 (D)) the second class (2 to 4 day is actually above the first, instead of smaller as calculated. On the other hand the fit of th longer sequences 4 to 13 and 14 to 40 days is quite good. The departure from the calculat seems to be due to the transfer of a number of single days into the 2 and 3 day groups.

3. Records at four other localities

In 1916 Newnham brought forward similar sets of data for four localities : Aberdee Kew and Valentia, each for the ten years 1901 to 1910, and Greenwich for 27 years 1887 1913. I have examined these figures and the results are summarized in Table II.

TABLE II. Summary of frequency distributions of wet and of dry spells at four localities fro original data of Newnham (1916), together with values calculated from the logarithmic seri

	Aberdeen 1901-1910		Kew 1901-1910		Valentia 1901-1910		Greenwich 1887-1913	
Wet								
No. of days	2180		1724		2602		4469	
No. of sequences	653		698		439		1769	
Average days per sequence	3·34		2·47		5·93		2·53	
Value of x	0·873		0·798		0·945		0·805	
No. of days in sequence	obs.	cal.	obs.	cal.	obs.	cal.	obs.	cal.
1	213	276	313	348·3	116	143·1	725	871·0
2-4	278	236·2	291	257·1	141	140·4	818	652·4
5-13	153	119·9	92	87·5	134	105·6	220	230·5
14-40	9	20·9	2	5·2	46	50·0	6	15·0
Over 40					2			
Dry								
No. of days	1476		1929		1027		5390	
No. of sequences	651		696		439		1764	
Average days per sequence	2·27		2·77		2·33		3·06	
Value of x	0·768		0·832		0·778		0·855	
No. of days in sequence	obs.	cal.	obs.	cal.	obs.	cal.	obs.	cal.
1	333	342·0	318	325·0	243	228·0	751	781·6
2-4	247	237·5	271	256·6	146	161·5	663	668·6
5-13	70	68·6	96	104·6	48	49·2	315	298·2
14-40	1	3·0	11	9·8	2	2·3	35	15·6

It will be seen that they support completely the conclusions drawn from the Harpende figures.

In the wet spells there are too few 1-day, too many 2 to 4-day and slightly too many 4 to 13-day sequences. In the dry spells on the contrary there are small differences only and they are not consistent in the four localities.

When the results of the four localities are converted into percentages (*i.e.*, distribution 100 spells), and averaged, the results are shown in Table III and Fig. 2. It is seen that in dry spells the calculated results differ at the most by 0·2 per cent from the observed. The t spells, on the other hand, show that the differences from the log series are consistent.

ABLE III. Average percentage of spells of different lengths of wet and of fine days in the four localities shown in Table II

. of days in equence	Wet		Dry	
	obs. (per cent)	cal. (per cent)	obs. (per cent)	cal. (per cent)
1 ..	36·2	43·5	48·6	48·8
2 - 4 ..	40·7	35·5	36·9	37·0
5 - 13 ..	19·9	17·0	13·3	13·4
Over 13 ..	3·3	4·0	1·1	1·0

4. The difference between 'wet' and 'dry' sequences

There are two possible explanations of the difference between the dry and wet sequence stribution; either that the meteorological conditions producing rain are, once they have rmed, liable to be stable for a period of two or three days; or that the results are due to me difference in the form of the observation or the method of observing. This latter ernative must be discussed before we need consider the former.

In comparing the terms 'wet day' and 'dry day' it is important to note that 'one dry y' means at least 24 hr and perhaps up to 48 hr without rain; and 'two dry days' means least 48 hr and perhaps up to 72 hr without rain. On the other hand, 'one wet day' may ean anything from 1 min to 24 hr rain, and 'two wet days' may be produced by a few inutes' rain if it happens to be falling at the purely artificial hour of separation (0900 GMT) tween successive days. Two short spells of 'wet' days can be joined together unto one nger one by a single short shower.

Thus by the terms of definition of wet and dry days there is a logical difference between e two; a very short spell of rain can easily turn one wet day into two – whereas the same is t true of the dry days. The longer the wet spell lasts, however, the less the results will be fected, and this has been found to be so in the close resemblance of the longer wet sequences observed and calculated series.

So it appears that there is good reason to suppose that the logarithmic series may represent e frequency distribution of 'dry' sequences of different length in a locality, and that the stribution of 'wet' sequences is somewhat similar, but there are differences which may partly explicable by differences of observation and definition.

The form of any particular logarithmic series is determined by the value of x, and this ould be approximately constant for different periods of observation in the same locality. the present records for Harpenden x for the dry periods was 0·856 for the first five-year riod and 0·839 for the second.

The value of x is dependent on the average number of units per group according to the rmula

$$N/S = x/[(1 - x) - \log_e (1 - x)]$$

Thus the form of distribution should be constant for all localities with the same average mber of days per sequence. It so happens that in the Harpenden data the average number days per sequence is very similar in both wet and dry series, but this is an accidental result the equality of the total number of wet and dry days.

Finally to return to the original starting point of this discussion, the chances of a sequenc lasting one further day are determined by the value of x ; and the following are two examples :

If $x = 0{\cdot}5$ then 28 per cent of first days will be followed by a second, 36 per cent c second days will be followed by a third, and 40 per cent of third days will be followed by fourth, etc.

If $x = 0{\cdot}9$ the percentage values are 61, 71·1 and 75·8 respectively.

In studies of the present type the value of α is dependent on the length of the observatio period. In the Harpenden figures it will be seen that the values of α for the ten-year period are almost exactly the sum of the two values for the five-year periods.

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551.556.2

The effect of a hedge on the flow of air

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Summary

Experiments are described in which the horizontal wind speed at three heights up to 2 m is measured various distances up and down wind from a dense hedge 1·68 m high on occasions with wind directions proximately normal to the hedge. The results show that the hedge begins to exert an influence on the nd speed at 2 m at a distance equivalent to approximately 15 hedge heights upwind, and that down- nd its influence extends to a distance approximately equivalent to 60 hedge heights. At the two lower ghts of wind-speed measurement the range over which the hedge has an effect is smaller. Diagrams constructed to show the decrease in wind speed at the various heights within the distance range ntioned above.

1. Introduction

A survey of the influence of a hedge of the type often found as a border to gardens and lds on the horizontal wind speed both upwind and downwind from the hedge was under- ken as a preliminary investigation to test the suitability of a site for experiments concerned th certain aspects of the turbulent exchange process. The results of this survey may hold me interest for those concerned with the microclimatological effects of such obstacles.

2. The site and apparatus

The site was a rectangular sports field covered with a surface of short grass and surrounded a hawthorn hedge. The field adjacent to this sports field on the latter's south and west des was covered with wheat stubble throughout the period of the experiments. Along the est side of the sports field the hedge was somewhat open from the ground to a height of cm, but from this level to its top it presented a very dense barrier to the flow of air. The dge was kept well trimmed throughout the period of the experiments and its height, which as well defined, decreased fairly uniformly from 2 m at the NW. corner of the sports field 1·5 m at the SW. corner. A point B was selected on this hedge where it had a height, h, 1·68 m, and a line was marked on the ground normal to the hedge at this point.

The instruments employed were sensitive cup anemometers similar to those described Sheppard (1940). They were calibrated in a wind tunnel during the course of the survey. asts were available on which the anemometers could be exposed at three heights, z_1, z_2, d z_3 equal to 2, 1, and 0·5 m respectively.

3. Measurement of the unobstructed wind

The purpose of the measurements was to determine the distances upwind and downwind om the hedge at which anemometers could be exposed and be expected to yield results hich would not be influenced by the presence of the hedge. A further aim was to obtain me indication of the pattern of horizontal wind speed between these positions and the hedge. o this end it was necessary to expose anemometers at various distances from the hedge and compare their readings with the free or unobstructed wind. A first step was to determine here the free wind should be measured. Measurements were confined to occasions with esterly winds which were within 15° of the normal to the hedge, and a few preliminary easurements showed that, as far as the upwind direction was concerned, no difficulty arose anemometers sited at a point C which was 30·5 m from the hedge gave the same results o to 2 m) as anemometers sited further from the hedge. The free wind was therefore easured at this position when investigating the upwind effect of the hedge.

Downwind from the hedge there was a uniform surface of short grass for 147 m and then econd hedge which formed the eastern boundary of the sports field. As far as possible it s necessary to choose a point within this 147 m at which neither hedge had an effect on the ttern of the horizontal wind speed. After a few observations it was decided to measure

the free wind when working on the downwind side of the hedge at a point A, 107 m from the point B (Section 2). It will be seen later that this choice was a compromise in so far as there was an indication that the hedge might still have a small effect on the wind speed at 2 m in this position.

4. Experimental procedure

Twelve anemometers were available so it was possible to use no more than 4 masts at any one time. One mast was therefore kept stationary at either position A or C and the other three were moved along the line AB or BC as required. The mast which was placed at either position A or C will be referred to as the reference mast.

The procedure for any one experiment was as follows :

The 4 masts were set up with 3 anemometers on each. The anemometers were started at 5-sec intervals and allowed to run for 15 min. They were then interchanged in such a way that the anemometer exposed to the highest wind at a particular height was afterwards exposed to the lowest wind at the same height. In practice this involved changing the anemometer at 2 m on the reference mast, i.e., the mast furthest removed from the hedge and therefore exposed to the highest wind, with that at 2 m on the mast nearest to the hedge, interchanging the anemometers at 2 m on the intermediate masts, and repeating the process at the other two heights. A second 15-min run was then taken and the individual wind speeds at each height in each position during the two runs meaned. In this way it was hoped to minimise any instrumental error.

A series of observations was performed in which the spacing between the masts as they were moved along either the lines AB or BC was reduced as the hedge was approached. In this way the wind speeds at the three heights in a number of positions within the limits set by the appropriate reference mast position and the hedge were obtained together with the wind speeds at the same three heights at the reference position in every case.

5. Method of presentation of the results

Since the measurements were made on a number of occasions the absolute values of the wind speeds at the reference positions varied. The wind speeds at the three heights in the various positions were therefore expressed as a percentage of the wind speeds at the corresponding heights at the reference position. The results upwind and downwind from the hedge are set out in Tables I and II respectively, the distances from the barrier in terms of its height being shown therein, see e.g., Panfilov (1937).

These results are also shown in Figs. 1 and 2, in which the distance scales are logarithmic

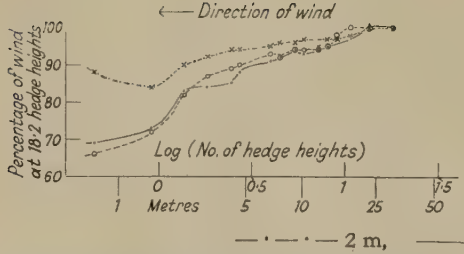

Figure 1. Curves show wind speed at 2, 1, 0·5 m as percentage of wind speed at the corresponding height at 18·2 hedge heights (30·5 m) upwind from the hedge.

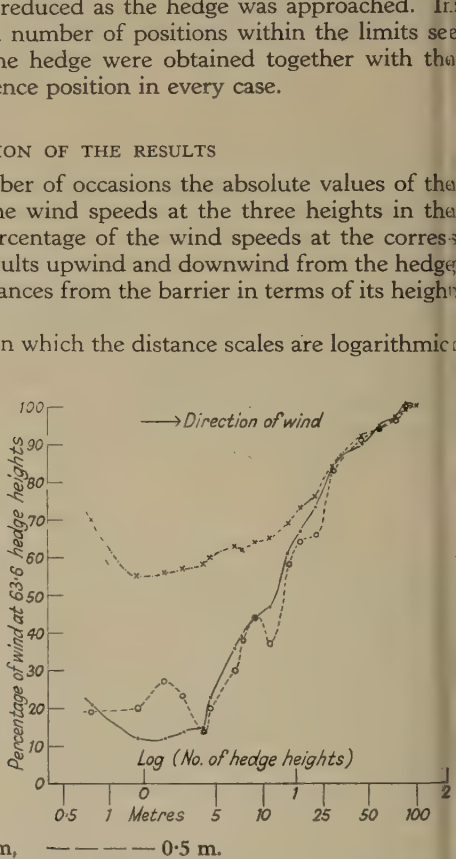

Figure 2. Curves show wind speed at 2, 1, 0·5 m as percentage of wind speed at the corresponding height at 63·6 hedge heights (107 m) downwind from the hedge.

— · — · — 2 m, ——— 1 m, — — — — 0·5 m.

TABLE I. HORIZONTAL WIND SPEED UPWIND FROM THE HEDGE AS A PERCENTAGE OF THAT AT THE SAME HEIGHT AT 30·5 M (18·2 HEDGE HEIGHTS) UPWIND FROM THE HEDGE

Distance from hedge		Heights of wind-speed measurement		
metres (x)	hedge heights (x/h)	$z_1 = 2$ m	$z_2 = 1$ m	$z_3 = 0{\cdot}5$ m
0·76	0·45	88	69	66
1·52	0·91	84	73	72
2·29	1·36	90	83	82
3·05	1·82	92	84	87
3·82	2·46	94	85	89
4·57	2·73	94	88	90
6·10	3·63	95	91	93
7·63	4·50	96	92	92
9·16	5·46	96	94	94
10·7	5·99	97	93	94
12·2	7·26	95	94	94
13·7	8·15	97	95	95
15·3	9·09	97	97	98
18·3	10·8	98	97	100
22·9	13·6	100	101	100
30·5	18·2	100	100	100

Ratio of wind speeds at 30·5 m from the hedge for all occasions of observation :

z_1 (2 m)	z_2 (1 m)	z_3 (0·5 m)
100	89	75

TABLE II. HORIZONTAL WIND SPEED DOWNWIND FROM THE HEDGE AS A PERCENTAGE OF THAT AT THE SAME HEIGHT AT 107 M (63·6 HEDGE HEIGHTS) DOWNWIND FROM THE HEDGE

Distance from hedge		Heights of wind-speed measurement		
metres (x)	hedge heights (x/h)	$z_1 = 2$ m	$z_2 = 1$ m	$z_3 = 0{\cdot}5$ m
0·76	0·45	70	21	19
1·52	0·91	55	12	20
2·29	1·36	56	12	27
3·05	1·82	57	14	23
3·82	2·46	58	15	14
4·57	2·73	60	23	20
6·10	3·63	63	36	30
7·63	4·50	62	39	38
9·16	5·46	64	44	44
11·4	6·80	65	47	37
15·3	9·09	69	61	58
18·3	10·8	73	67	64
22·9	13·6	76	73	66
30·5	18·2	84	84	83
45·9	27·3	92	90	91
61·2	36·4	94	95	94
76·4	45·5	97	97	96
91·6	54·6	99	101	100
107·0	63·6	100	100	100

Ratio of wind speeds at 107 m from the hedge for all occasions of observation :

z_1 (2 m)	z_2 (1 m)	z_3 (0·5 m)
100	92	83

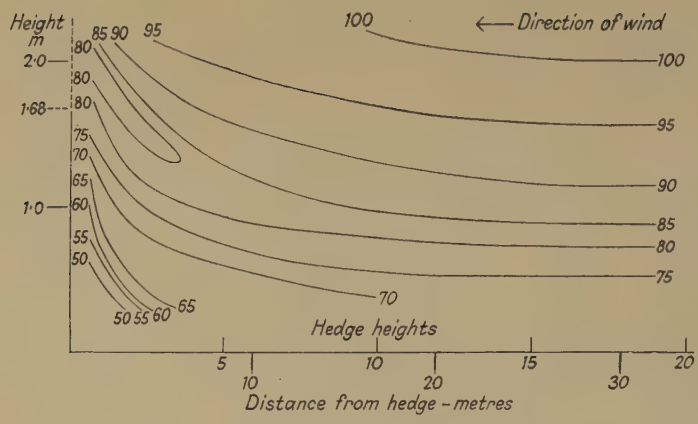

Figure 3. Vertical section lines of equal wind speed upwind from the hedge. Percentages are of wind speed at 2 m 18·2 hedge heights (30·5 m) from the hedge.

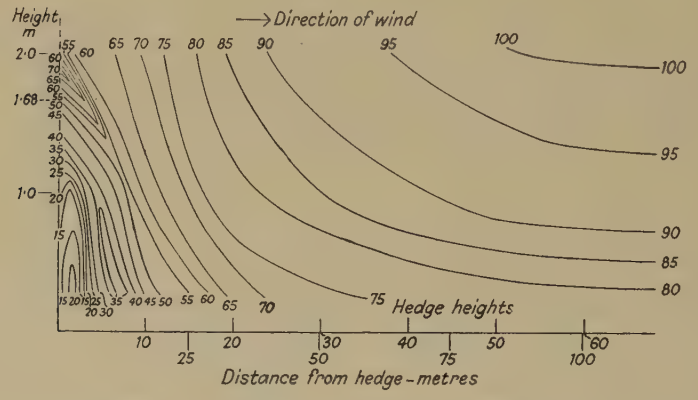

Figure 4. Vertical section showing lines of equal wind speed downwind from the hedge. Percentages are of wind speed at 2 m 63·6 hedge heights (107 m) from the hedge.

Figs. 3 and 4 show on a vertical section, lines of equal horizontal wind speed, upwind and downwind from the hedge respectively, in terms of the free wind at 2 m in the appropriate reference position. This was done in the following way. The values of the wind speeds at the three heights for all occasions of observation at each reference position were meaned and the ratios of the horizontal wind speeds at 1 m and 2 m and similarly at 0·5 m and 2 m for the upwind and downwind reference position obtained. The percentages shown in Tables I and II for the 1 m and 0·5 m heights were then reduced in these ratios. As was expected, these ratios were not the same for the two positions since the characteristics of the two surfaces differed. The surface of the sports field was unusually smooth for a natural surface and had a grass length of about 3 cm. The surface of the other field was rough and the length of the stubble was about 15 cm. There were also a few taller weeds.

6. Examination of the results

(*a*) *Upwind from the hedge*

The figures show that the effect of the hedge first appeared at the 2 m and 1 m heights at about 15 hedge heights in front of the hedge. At the 0·5 m height the corresponding distance was approximately 10 hedge heights. At the 2 m height there was a steady decrease in the

wind speed as the hedge was approached and finally an increase. For the other two heights the decrease continued up to the nearest point of measurement, the decrease becoming more rapid as the hedge was approached, except that very near the hedge the rate of decrease diminished. It would be expected that if the hedge had been dense right down to the ground the decrease at 0·5 m would have been more marked.

(*b*) *Downwind from the hedge*

The most striking feature of the results is the indication that the effect of the hedge extended to about the same distance for the 1 m and 0·5 m heights. This was unexpected as it has been generally accepted that for a height z objects or changes in surface characteristics outside a distance of 100 z will have no influence on the measured wind speed at z. We should therefore have expected that after a fetch of about 50 m (30 hedge heights) there would be no further increase in the measured wind speed at 0·5 m, whereas the further increase was in the region of 10 per cent. At 1 m the wind speed appeared to have attained its unobstructed value well within the expected distance. There is an indication that the wind at 2 m may not have attained its unobstructed value at the furthest point of measurement, but it certainly must have almost done so. It should be remembered that the hedge was only 1·68 m high, and the 2 m level was well above its top.

At 1 m and 0·5 m there appeared to be two positions at which the wind speed reached a minimum value, and at a distance of approximately 2 hedge heights the absolute value of the wind speed at 0·5 m was greater than that at 1 m. These effects were due to the interaction of the air coming over the top of the hedge and that coming through the hedge at its relatively open base.

(*c*) *General*

The available literature on the effects of the wind breaks indicates that the effectiveness of a barrier depends on the absolute value of the free wind, the effect extending to greater distances with increasing wind speed. No attempt has been made here to investigate this statement as a much more comprehensive survey would be required. The mean wind speed at 2 m for the downwind reference-position was 3·64 m/sec, with extremes of 2·57 m/sec and 5·54 m/sec. The corresponding values for the upwind reference position were 2·98 m/sec, 2·32 m/sec and 4·15 m/sec.

One might expect the stability of the atmosphere within the layer investigated to have some small effect on the detailed pattern. Although no measurements of temperature gradients were made during the course of these observations, all occasions had overcast skies and a moderate wind speed and it is certain that the stability did not differ greatly from neutral.

The detailed pattern of the lines of equal wind speed near the hedge as shown in Figs. 3 and 4 is to some extent imaginary, but the lines have been inserted as representing the most likely pattern indicated by the measurements that were made.

7. Acknowledgments

I am indebted to the Master and Fellows of Downing College, and to Mr. R. Cornwell of Clay Farm, Cambridge for permission to carry out this work on their lands. I also wish to thank the Director of the Meteorological Office for permission to submit this paper for publication.

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551.524.36(42)

A comparison of the daily extremes of temperature at two stations in a Chiltern dry valley

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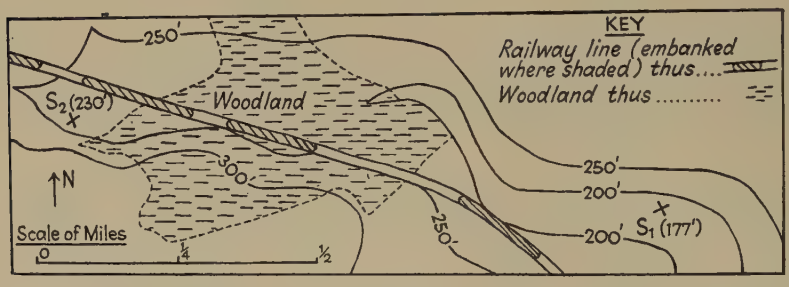

Figure 1. Sketch map of the eastern part of the valley showing the positions of the stations S_1 and S_2.

The valley ' rises ' at Little Chalfont in Buckinghamshire and winds gently down for 4 mi to join the Colne river valley at Rickmansworth in Hertfordshire. Station S_1 (see Fig. 1) is situated in a bend of the valley and is particularly hemmed in to the N. and NE. The climate here has been fully described by Hawke (1944) and shown to be the most continental investigated in Britain. Station S_2, 1·2 mi up the same valley and 50 ft higher, is less enclosed, the steepest and nearest slope being to the south. The largest diurnal range at S_1 in 21 years' records was 52·1°F (max 90·8, min 38·7) on 15 Aug. 1949. On this date the range at S_2 was only 43·3° (max 84·5, min 41·2). The largest diurnal range at S_2 in five years' observations was 47·5° (max 88·6, min 41·1) on 12 July 1949. Table I shows that the up-valley site S_2 is less continental than S_1, the mean maximum temperatures being lower and the mean minimum temperatures higher throughout the year. Only on rare occasions does S_2 experience greater extremes than S_1.

TABLE I. MEAN AND EXTREME MAXIMUM AND MINIMUM TEMPERATURES RECORDED AT S_1 AND S_2 IN PERIOD AUG. 1946 TO JULY 1949 (IN °F)

			Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.
mean	max	S_1	46·0	43·9	51·5	61·0	66·5	70·6	75·3	74·1	70·3	59·7	52·0	46·4
	max	S_2	44·4	42·3	50·7	59·7	65·3	69·4	73·5	(72·7)	67·7	57·6	50·1	44·5
mean	min	S_1	28·9	25·5	29·0	34·3	38·2	43·9	49·3	47·4	44·7	36·5	33·1	30·2
	min	S_2	30·3	28·1	31·4	36·1	39·4	45·6	50·4	(48·7)	46·1	38·3	35·9	31·9
highest	max	S_1	58	63	73	86	92	94	95	92	85	74	66	57
	max	S_2	56	60	71	81	87	91	92	90	81	70	61	56
lowest	min	S_1	− 4	− 3	1	22	22	30	35	32	27	17	14	− 2*
	min	S_2	− 3	− 3	4	22	25	32	36	—	29	19	16	6*

* Large difference attributable to variable snow cover.

The lower minimum temperatures recorded at S_1 are probably due in part to the greater protection from wind disturbance afforded to cold air pockets there. Table II gives the mean excess $M(\Delta m)$ of the minimum temperature recorded at S_2 over that recorded at S_1 for each wind direction and for calms. In order to test more effectively the influence of wind direction on $M(\Delta m)$, only nights were considered on which the wind (recorded at Rothamsted 12 mi NNE. of stations, elevation 420 ft) was the same at 21 hr and 09 hr. Table II shows : (i) that the minimum temperatures recorded at S_1 and S_2 are significantly more nearly the same on calm than on windy nights [$M(\Delta m)$ (for all except calm) $-$ $M(\Delta m)$ (for calm) $= 3{\cdot}1\,\sigma_D$ where σm = standard deviation of $M(\Delta m)$. σ_D = standard deviation of the difference between means]. (ii) that the minimum temperature recorded at S_1 is significantly lower with respect to that recorded at S_2 when the wind is from the N. or NE., directions in which S_1 is particularly well protected from wind disturbance, and S_2 not at all [$M(\Delta m)$ (for N. and NE.) $-$ $M(\Delta m)$ (for all except N. and NE.) $= 4{\cdot}5\,\sigma_D$]. (iii) that $M(\Delta m)$ follows closely the expected relative protection of sites S_1 and S_2 as inferred from Fig. 1 for the remaining wind directions.

TABLE II. VARIATION WITH WIND DIRECTION OF THE MEAN EXCESS, $M(\Delta m)$, OF THE MINIMUM TEMPERATURE RECORDED AT S_2 OVER THAT RECORDED AT S_1

	N.	NE.	E.	SE.	S.	SW.	W.	NW.	Calm	All except Calm	N. and NE.	All except N. and NE.
$M(\Delta m)$	2·9	2·8	2·1	1·8	1·5	1·8	2·1	2·3	1·3	2·2	2·9	1·9
								σm	·26	·13	·10	·20

The mean maxima recorded at S_1 are higher, probably because this site is near a sunny south slope, while S_2 is dominated by a north slope. No correlation between the difference in maximum temperatures recorded at S_1 and S_2 and wind direction was found.

In conclusion, this note establishes the existence, only 1·2 mi along the valley bottom from the well known Rickmansworth frost hollow (S_1), of a site with an appreciably more temperate climate. The role played by protection from wind disturbance in the production of freak valley climates is suggested.

ACKNOWLEDGMENT

The author wishes to thank Mr. E. L. Hawke for his kind encouragement and for making available the daily maximum and minimum temperatures at the Rickmansworth frost hollow (S_1).

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Correspondence

551.506.5 : 551.555.1

A proposed 'trade' expedition

By H. Riehl

No doubt it is unusual for an author to send in the first letter on his article. But certain thoughts regarding the paper, 'The north-east trade of the Pacific Ocean' (*Q.J.*, **77**, p. 598), did not seem to fit into the text proper, and yet I would like to bring them to the attention of the readers. These thoughts concern plans for a new trade-wind study, touched on briefly at the end of the paper.

We attempted to deal with two types of problems in the article, one large-scale, the other small-scale. The balance of heat, moisture and momentum in atmosphere and ocean for short and long periods requires gathering of data over considerable distances, at least several hundred miles. A study of the vertical exchange of property needs detailed observations over small areas, tens of miles or less. It would seem that any substantial improvement of detailed forecasting in atmosphere and ocean depends in considerable measure on improved understanding of both large- and small-scale aspects. I consider it advisable to give serious thought toward fitting out an expedition that would gather the requisite data. Our article could serve as a guide, at least partly, in establishing the type and routine of observations to be taken.

Without going into too much detail at this time, I believe that the tools to make the undertaking a success are available or can be developed. We had, for instance, no rainfall measurement. With radar techniques, or even raingauges, this gap could be filled easily. In view of recent instrumental advances, it should be possible to measure the heat exchange between ocean and air directly. There is a fair chance that the same can be done with radiation. It may even be possible to calculate the radiational heat flux at different levels with the use of aircraft. As a reasonable extension of our computations, parallel data can be gathered in the ocean.

The main difficulty is the large amount of equipment necessary to measure the horizontal and vertical flux terms in the large-scale problem. We have no instrument that measures the small mean vertical motions (order of mm sec^{-1}), and these must still be calculated from the continuity equation. Our paper has shown that the flux terms are far from negligible, and I can see no point in trying to recalculate the balances without knowing the divergence and vertical motion.

We had only one component of the divergence available. Any new undertaking evidently must measure both components. This requires the use of at least 3 ships, arranged in triangle formation across the current. If this is done, however, one can no longer measure the changes of property along the air trajectory. As I see it, a minimum of 5 ships is requisite, arranged perhaps as shown below. The spacing between the 3 ships along the trajectory would be about 350 mi ; the distance of the ships situated normal to the current from the centre ship would be about 250 mi. This formation would take data for 6 to 8 weeks. Since the ships could not remain at station continuously for such a period, two or three observation periods will be needed, with intermittent time in port.

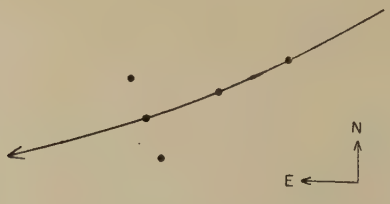

Figure 1

The small-scale programme will take much less time, perhaps one week, since a much ger number of small-scale events occurs in a given time interval. For this purpose the ips would be arranged in a cluster, with the distance between ships only a few miles, some- nes less. Three aircraft should suffice to make horizontal and vertical runs above the ships probe variations of the inversion structure over short distances, to study the subcloud and oud layers, and further investigate the life cycle of cumulus clouds. These aircraft would ake radiation and inversion studies during the large-scale programme. They would also vestigate special problems, such as the measurement of cloud cover.

The north-east Pacific is a suitable location for the proposed expedition, since very equate machine shop facilities, etc., exist in Honolulu. Any other trade region with similar cilities would, of course, serve equally well. I should like to recommend seriously that e outlined undertaking be carried out as an international project.

epartment of Meteorology,
niversity of Chicago.
November 1951

551.501.7 : 551.557.2

A NOTE ON THE DISTRIBUTION OF FREE-AIR WIND VECTORS ABOUT THEIR MEAN

By J. W. HUTCHINGS

In a recent work Brooks and others (1950) have put forward the hypothesis that in the per air, away from the influence of surface features, the individual wind vectors are distri- ited about their mean in accordance with the normal law of errors. Their examination of extensive amount of upper-wind data tended to confirm this hypothesis, and there is tle doubt that, in the absence of long-period wind-frequency data, the assumption of a rmal distribution would yield a valuable approximation to the true distribution of winds. or some wind régimes there is an obvious limitation to the occurrence of such a distribution it, as pointed out by Brooks and others, such limitation can usually be overcome by a suitable oice of the period over which the average is taken. Brooks and others thus tend to regard e distribution as largely empirical.

Nevertheless, it seems worth while to point out that, even when the averaging period is itably chosen, the assumption that an increasingly large number of observations would eld a distribution approximating more and more closely to the proposed normal distribution, ads to theoretical difficulties. To see this, we notice that at any one station and at any parti- lar level the poleward flux of relative eastward zonal momentum is given by the average er a given period of the quantity ρuv, where ρ is the density and u, v are the eastward and oleward components of wind. As mentioned by Widger (1949) and others the error committed considering the density at a given level as constant is not important in connection with udies of momentum transport in the atmosphere. With this assumption the flux of relative nal linear (and angular) momentum is then proportional to the mean value of uv over the ven period. By writing

$$u = \bar{u} + u' \qquad v = \bar{v} + v'$$

bar denoting the mean value of a variable over a given time, we deduce

$$\overline{uv} = \bar{u}\,\bar{v} + \overline{u'v'}$$

owing that the total momentum flux over the period can be separated into a local advective ux (proportional to $\bar{u}\,\bar{v}$) and a local eddy flux (proportional to $\overline{u'v'}$).

But the distribution of vector deviations proposed by Brooks and others is entirely sym- etrical and for every combination (u', v') there exists a corresponding combination $(u', -v')$, that the associated values of $u'v'$ are equal in magnitude but opposite in sign. For such a stribution, therefore, the time mean value of $u'v'$ is identically zero and this circumstance ads to zero eddy transport of momentum at that station and level. This conclusion is in anifest conflict with modern evidence that such eddy transport is appreciable and is in fact considerable importance in maintaining the angular-momentum balance of the atmosphere riestley 1949, Widger 1949, Starr and White 1951).

In considering the nature of the differences between long-period wind roses and th theoretical distributions, e.g., Larkhill at 500 mb, (Brooks and others p. 8), it may well be tha at least some of these differences (described by Brooks and others as ' within the limits o error of observed wind roses ') are due to the essentially asymmetrical distribution of vecto deviations necessary to effect the required meridional transport of angular momentum withi the atmosphere.

Meteorological Office,
Wellington, N. Z.
9 August 1951

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Starr, V. P. and White, R. M. 1951 *Ibid.*, **77**, p. 215.
Widger, W. K. 1949 *J. Met.*, **6**, p. 291.

Reply

By J. S. Sawyer

As Mr. Hutchings points out, if the distribution of wind vectors about their mean wa exactly normal and circular then the large-scale disturbances of the atmospheric circulatio would produce no net flux of angular momentum from the equator to the pole. Priestley (1949 and 1951) and others have demonstrated the importance of this eddy flux of angula momentum in the mechanism of the general circulation and its existence requires a correlatio between the two components of velocity u and v (notation as used by Mr. Hutchings). Th correlation coefficient can be roughly calculated from the values of $\overline{v'u'}$ given by Priestley and the standard vector deviations σ of wind in *Geophysical Memoir* No. 85, and it is foun that the correlations are quite small. At Larkhill the greatest values at any level or seaso are 0·2. Values at Gibraltar and Habbaniya may be as large as 0·3 at some levels and somewha higher values are suggested at Auckland at 200 mb and 150 mb. However in view of th uncertainties of the estimates of σ in *Geophysical Memoir* No. 85 in the southern hemisphere and the small number of data used it would be desirable to return to the original observation to calculate the correlations at Auckland and much weight should not be given to the larg values there.

The purpose of *Geophysical Memoir* No. 85 was to provide a basis for estimation of the upper-wind frequency at any point of the globe. In view of the small correlations betwee u and v, the two-parameter model adopted appears to be quite adequate and certainly n more complicated model of the wind-frequency distribution at a point would have been prac ticable or justified in the present state of knowledge of upper-air climatology.

Meteorological Office,
Dunstable.
26 September 1951

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1951 *Ibid.*, **77**, p. 200.

551.521.3, 551.524.4, 551.551, 551.574.4

Turbulent-transfer coefficients near the ground

By C. H. B. Priestley

After its recent airing in this *Journal* the question of equality or inequality of the eddy-nsfer coefficients near the ground might best be left to await the outcome of direct flux asurements now in progress. But this will take some time and not a little trouble, and anwhile fresh evidence has been brought forward by Rider and Robinson (*Q.J.*, **77**, p. 375) he form of wind, temperature and humidity profiles in the lowest 2 m. These are inter-ted as indicating equality of the coefficients, an impression which may tend to stick because s based on that most memorable of all appeals, the visual one. To prevent this happening, independent appraisal of the evidence is called for.

The case for equal coefficients in this instance rests on the alleged identity of the corres-nding profiles. In support the authors give a table showing the mean ratio of the slopes the temperature and humidity profiles, determined by eye from curves fitted by eye. The io so obtained shows no appreciable variation with height. The averaging process embraces conditions without differentiation as to stability.

A more relevant and objective test can be made by : (i) discriminating between lapse d inversion conditions and (ii) using finite differences

$$F(z)=\frac{\Delta e}{\Delta T+\Gamma.\Delta z}$$

tained from the actual measurements instead of the tangents to curves arbitrarily smoothed llowing the authors, e is used for vapour concentration). It is unfortunately not possible make a similar test of the conformity of the wind profile as the wind observations were not the same levels. The observations at 4 and 100 cm were reported as least reliable but the naining levels allow F to be computed over three ranges of comparable height ratio, i.e., to 8, 50 to 14, and 200 to 50 cm. Ignoring the Γ term for the moment, the mean value of from 14 of the 15 lapse occasions (18.6.49 is in error or a freak) were 2·36, 3·07 and 3·32 in three ranges respectively, in arbitrary units. The average values of the ratios of the second d third respectively to the first were 1·34 ($\pm$ 0·08) and 1·51 ($\pm$ 0·12), the figures in brackets ing the (normal) standard error of the mean. Thus the evidence, at a high level of signi-ance, points to a steady increase of F with height (which would have been slightly greater the Γ term had been included). The sense is such as to imply $K > D$ and so agrees with findings of Pasquill and the preliminary results of flux measurements by Swinbank ; the gnitude looks about right also, bearing in mind that the influence of site may prove appreciable.

In the inversion trials the validity of the individual profiles was harder to appraise and mean F showed no significant change with height, again in sympathy with the findings other workers.

Robinson and Rider discriminate with regard to stability only in their second test, where ofiles plotted on a linear scale are superimposed and a single curve drawn to fit. The results visually impressive, but there is no discussion as to what tolerance is allowable. The gures provided above show that in fact this tolerance is very small and the simple test quite adequate. A curve of typical profile shape with a thirty-fold increase in slope between p and bottom looks very like another with a more than forty-fold increase ; in Fig. 2 as produced two such curves could be superimposed without any visible space between.

It is hoped that this letter will draw wider attention to the care required in making in-rences from the general appearance of the profiles, for Rider and Robinson are not the st to have done so without an objective procedure and proper tolerance checks. In such a se as is treated here it is clear that extreme delicacy is required, and the emergence of a gnificant result in this instance is a tribute to the quality of the measurements.

ustralian Scientific Liason Office,
frica House,
ondon, W.C.2.
October 1951

551.521.3, 551.524.4, 551.551, 551.574

Eddy diffusion of heat and water vapour

By F. Pasquill

In a recent paper entitled ' A study of the transfer of heat and water vapour above a surface of short grass ' (*Q.J.*, **77**, p. 375), Mr. N. E. Rider and Dr. G. D. Robinson have given an account of measurements which represent a welcome addition in a field of investigation which depends on precise instrumental observations of a comprehensive character. They have considered the important question of the relative magnitudes of the apparent eddy diffusivities for heat and water vapour in non-adiabatic conditions and have concluded that most of their results support the classically assumed identity of these diffusivities.

Observations which I have described elsewhere (*Proc. Roy. Soc.*, A, **198**, p. 116, 1949) indicated eddy diffusivities for heat greater than those for water vapour in unstable conditions and provided the first direct evidence in support of a feature predicted on theoretical grounds by Priestley and Swinbank (*Proc. Roy. Soc.*, A, **189**, p. 543, 1947), namely, that the processes of turbulent exchange in a thermally inhomogeneous atmosphere may produce effects which impose different numerical values on the classically defined eddy diffusivities for various transferable properties. However, there is no reason so far, either theoretically or experimentally, to expect these differences to be a permanently occurring feature. For such differences to exist the theory requires a correlation between the local density of the transferring medium and the properties undergoing transport. As I have previously argued (*Met. Mag.*, **79**, pp. 83–84, 1950), a qualitatively suitable correlation between temperature and vapour pressure could well have existed above the patchy grass cover over which my observations were made. Generally speaking, it is difficult to avoid the idea that the relative magnitudes of the various eddy diffusivities in the air near the ground must be influenced, if not completely determined, by the relative distribution of the thermal, evaporative and aerodynamic qualities of the underlying surface. Furthermore, it is to be noted that Rider and Robinson's profile data indicate that their observations are probably representative of horizontally homogeneous flow over the immediate surface involved only in the region below about 30 cm, whereas the results which I obtained were derived for a height of 75 cm. Finally, it should be remembered that the most direct and critical examination of the point at issue is provided by the determination of the simultaneous fluxes of heat and water vapour either by appeal to heat-balance consideration, as in my results, or by direct measurement of the turbulent flux as reported by Swinbank in a recent number of the *Journal of Meteorology*. Rider and Robinson's results do not specify the separate flux components and they necessarily rely on indirect arguments.

On several grounds therefore it is not necessarily surprising that Rider and Robinson's conclusions show some divergence with those which I drew. I would, however, like to offer some comment on a particular feature of their data and argument. As they state in their conclusions, their main argument is based on the agreement shown in fluxes computed on the one hand by applying the classical heat-balance approach, with equality assumed in the diffusivities for heat and water vapour, and on the other hand, by an aerodynamic approach which has been given independent empirical justification. But it is noticeable that among the 13 occasions represented in their results (see their Fig. 3), there is a group of five (20 June 1121 and 1410, 21 June 1111 and 1400, 1 July 1428) for which the vapour flux computed by the heat-balance method is greater than that given by the aerodynamic method applied at a height of 10 cm to an extent which is more than can be accounted for by the estimated limits of error in the combined sensible- and latent-heat flux. In view of the dependence shown by my results on the Richardson number it seemed of interest to consider the magnitude of this parameter for these five occasions, in relation to those obtaining on the remaining occasions. The relevant values of the parameter have been obtained from a previous reporting of this work by the same writers (a paper of the Meteorological Research Committee, London, *M.R.P.* 547). From this it appears that the Richardson numbers (at 10 cm) for the above occasions range from -13 to -43×10^{-3}. Of the other eight occasions, only one (24 June 1419) corresponds to a Richardson number in this range (actually -26×10^{-3}), while the remainder are associated with magnitudes between -6×10^{-3} and $+33 \times 10^{-3}$.

It is evident, therefore, that in the relatively unstable conditions as defined by Richardson nber, the *heat-balance* estimates are on most occasions greater than the *aerodynamic* esti- es ; if the latter estimates can be accepted as true then the evidence is in favour of $K/D > 1$ hese conditions, where K and D are the eddy diffusivities for heat and water vapour respec- ly. In this connection it will be recalled that Rider and Robinson point out that extra- ation of the aerodynamic estimates to zero value of z or Richardson number improves the ement in the estimates in most cases. However, it appears from the details given else- re (*M.R.P.* 547) that among the five occasions which provide the main discrepancies in two estimates of flux only two (20 June 1121 and 21 June 1111) do, in fact, indicate a able increase in the aerodynamic estimate in the height range from 25 to 10 cm. One asion (20 June 1410) is somewhat doubtful owing to the scatter of the values, while the aining two (21 June 1400 and 1 July 1428) show a decrease.

It seems therefore that the precise extent to which the aerodynamic estimates in the tively unstable conditions are truly representative of the vapour flux is at least a matter some speculation and any conclusion as to the corresponding relative magnitudes of nd D is accordingly open to some doubt. It is interesting to note, however, that the values K/D which would provide exact agreement in the heat balance estimates and the aero- amic estimates at $z = 10$ cm for the six relatively unstable occasions range from 0·8 to with a mean of 2·0.

This letter has been submitted with the permission of the Director of the Meteorological ice.

alth Physics Division,
omic Energy Research Establishment,
rwell, Berks.
October 1951

Reply to C. H. B. Priestley and F. Pasquill

By N. E. Rider and G. D. Robinson

We are glad that Mr. Priestley has put on record his analysis of our observations. We not entirely disagree with his strictures on our own ' visual ' methods ; even so we are changed in our opinion that, with observations of this imperfect quality, the visual method preferable to the statistical. Our argument is that we give much more weight to ' good ' servations than to ' poor ' ones, defining good and poor in the following way. It is sumably common ground that we are not concerned with an inherently random variable, , that the profile of mean humidity, correctly observed, is a smooth curve ; this seems to implied by the definition of the eddy diffusivity. We therefore suggest that humidity servations through which a unique smooth curve can easily be drawn are ' good ' observa- ns. If this cannot be done the observations are ' poor.' Good observations were unfortun- ly very few ; only 4 cases could be so described :

26 Nov. 1948 1745, inversion ; 20 June 1949 1400, lapse ; 21 June 1949 1100, lapse ; 24 June 1949 1415, lapse.

vo of these occasions are illustrated in Fig. 2 of our paper, the other two had been published viously (Robinson, *Cent. Proc. R. Met. Soc.*, p. 26, 1950). We have applied Mr. Priestley's thod to these four occasions with the results shown in Table I.

TABLE I. $|F(z)| = |\Delta\chi/\Delta T|$ IN G CM^{-3} °F^{-1} × 10^5* FOR VARIOUS HEIGHT RANGES (CM)

		4-14	8-25	14-50	25-100	50-200
Nov. 1948	1745	1·5	1·6	1·5	1·7	1·7
June 1949	1400	4·8	3·2	4·0	4·0	3·8
June 1949	1100	3·7	2·6	2·9	3·0	2·6
June 1949	1400	3·0	2·9	3·8	3·8	4·0

* This appears to be the unit used by Mr. Priestley. In our paper, and in Table II of this reply, $f(z)$ is the ratio of the centration gradient (cm^{-1}) and temperature gradient (°C cm^{-1}).

It is clear from this that the systematic variation of $F(z)$ noted by Mr. Priestley is entire due to the application of statistical methods to observations known to be in error. The fe observations which *may* be correct show no such variation.

Our case rests on three sets of facts :

(a) For ' good ' observations agreement between the humidity and temperature profil is very close indeed.

(b) On all other occasions the same profile can be drawn to fit both temperature an humidity observations *within the expected range of error of the humidity observations.*

(c) The mean f values, obtained as described in our paper, for occasions of lapse are given in Table II and show no systematic change with height. (The expected ratio $f(100)/f(1$ allowing for the effects of radiative flux is about 1·1). We suggest that it is not profitab to discuss the case of 2 Oct. 1948 at this juncture, and we ask to be allowed the luxury describing it as a freak ; it is probably, like 18 June 1949, just a particularly poor observatio

TABLE II. MEAN VALUES OF $|f| \times 10^4$ ($°C^{-1}$) AT VARIOUS HEIGHTS (CM), AND BY SUPERPOSITION PROFILES, IN LAPSE CONDITIONS

	7·5	13·5	25	50	100	Superpositio
16 Occasions	5·4	4·7	4·8	5·8	6·5	4·8
2 Oct. 1948	5·2	5·1	8·2	11·1	24	6·7
15 Occasions	5·4	4·6	4·6	5·4	5·3	4·7

Table II is clearly at variance with Mr. Priestley's figures. There are two possib reasons :

(a) The figures of Table II are obtained subjectively and may inadvertently be coloure by prejudice.

(b) The figures of Table II, but not Mr. Priestley's statistics, are obtained by a cor sideration of *all* the points on each profile in the light of the probable errors of measuremen both random and systematic (the systematic tendency to positive humidity errors is relevant

The great advantage of the statistical method, ruthlessly applied, is that it eliminates th exercise of unscientific prejudice ; its advocates sometimes forget that it may also eliminat the exercise of scientific judgment. We still think our interpretation of the observations i sound and that we have chosen the best method of examining them ; we agree that Mr. Priestle has shown that the evidence is not so conclusive as an uncritical reading of our paper migh suggest.

We cannot accept Dr. Pasquill's criticism without serious qualification. He mention 5 occasions (out of 13) when,

> the vapour flux computed by the heat balance method is greater than that computed by the aero dynamic method, applied at a height of 10 cm, to an extent which is more than can be accounte for by the estimated limits of error in the combined sensible and latent heat flux.

In our paper we are at pains to point out that there is no virtue in this height of 10 cm. I so far as the aerodynamic method can be said to have any empirical justification in super adiabatic conditions it is applicable only at zero height. We say,

> in most cases extrapolation to $z = 0$ improved the agreement with the flux as determined from th heat balance (with) two notable exceptions, 21 June 1949 1400 hr and 1 July 1949 1428 hr.

Two of the cases now quoted by Mr. Pasquill, 20 June 1949 1121 and 21 June 1949 1111 extrapolate to equality of the two methods at zero height, indeed we chose the latter for illu stration as an especially convincing case (Fig. 4a of our paper). The exceptional cases therefor reduce to the two which we ourselves noted, with one doubtful ; these are not the cases with the greatest magnitude of Richardson number.

So much talk about eddy-diffusion coefficients may leave the impression that we are convinced that there are valid theoretical grounds for believing their physical reality. This is not so, as we hope we made clear in our paper. The question to us is : do eddy-diffusion coefficients offer a useful empirical method of determining fluxes from observations made

ear the ground ? We suggest that the answer is ' yes ' only if the same coefficient can be sed for heat, water, and possibly momentum. If it cannot there seems to be no practical alue in the idea of the eddy diffusion coefficient, and failing a sound demonstration that the ertical flux of each quantity is proportional to the vertical gradient of that quantity, and to o other function of its distribution in space, the idea could with advantage be abandoned.

Kew Observatory,
November 1951

551.515.1

Large-scale vortex motion in the atmosphere

By R. W. James

May I make a brief comment on Dr. Goldie's letter (*Q.J.*, **77**, p. 455) on vortex structure ? Dr. Goldie quotes as the total kinetic energy of a unit cross-section of a Rankine vortex with cut-off at radius S as,

$$K = \tfrac{1}{4}\pi\rho R^2 + \pi\rho R^2 \log_e (S/R)$$

This was the expression given in my original paper (*Q.J.*, **76**, p. 255) but a simple examination of the dimensions reveals that it is wrong as it stands. The expression on the right should be multiplied by v_m^2, where v_m is the maximum speed at the spread radius, R. With this factor included it will be seen that the right-hand side will have the correct dimensions.

Dr. Goldie remarks that a sudden cut-off seems to correspond with what is observed in some lows studied by him. I do not think however, that the evidence is absolutely conclusive in favour of the cut-off as a characteristic morphological feature of occluded depressions. A good deal of synoptic evidence is required before the reality of the cut-off can be regarded as firmly established.

I believe it is fruitless at this stage to discuss whether the cut-off Rankine vortex provides a better or worse fit than some other model to an actual occlusion. Such are the data available that statistical tests will not discriminate absolutely between different models. In this situation it is permissible to select that model which is deemed more suitable for extraneous reasons. I selected the ' normal ' profile because, while giving a reasonable fit to observed profiles, it was mathematically more tractable.

Another point of interest raised by Dr. Goldie is the suggestion that on average the limiting radius of a vortex is about twice the spread radius. It is of interest to note that the ' sinoidal ' vortex has this property. Here again, statistical investigation is required to establish the mean value of S/R in depressions, and the standard deviation of departures from the mean in individual disturbances.

The discussion serves to underline that very little factual information is available on the morphology of occlusions, and that much research remains to be done.

10 Findhorn Place,
Edinburgh.
4 October 1951

551.515 : 551.513 : 551.509.3

Dynamics of flow patterns in extra-tropical regions

By A. Eliassen

In a recent article by Eady and Sawyer (1951) one finds among other things a brief discussion of Solberg's stability criterion for a circular vortex, or what is equivalent, for a straight

current on a rotating earth. Concerning the applicability of Solberg's theory, the authors make some statements which I find rather misleading, and on which I wish to comment.

In the first place, the authors fail to point out that this stability theory is restricted to perturbations which do not vary in the direction of the current. In other words, the stability criterion indicates, in the case of a circular vortex, whether adiabatic, *axially-symmetric* meridional circulations will have the character of stable oscillations or unstable overturnings. Thus the theory does not apply to all kinds of ' circulations in a vertical plane.' This point is relevant when Solberg's stability criterion is discussed in connection with the stability of wave disturbances such as depressions and anticyclones. Obviously, Solberg's 1936 theory does not apply in this case. There are, however, other reasons to believe that the stability properties for axially-symmetric perturbations are not completely unimportant for the growth of wave disturbances.

On the other hand, the authors seem to believe that the validity of Solberg's stability criterion is to a certain extent restricted because Solberg derived it from a particle method, assuming the pressure field to be unperturbed. In this connection I wish to call to their attention that Höiland (1939, 1941) has derived the same stability criterion without ignoring the pressure perturbations. Höiland made use of the circulation theorem, thus eliminating the pressure. The stability criterion in question is therefore valid quite generally for perturbations which do not vary in the direction of the current.

Stockholms Högskola,
Institutet för Meteorologi.
23 October 1951

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Höiland, E. 1939 *Arch. Math. Natur.* B, **42**, No. 5.
1941 *Norsk. Vid.-Akad. Mat. Natur.*, No. 11.

Reply

By E. T. Eady and J. S. Sawyer

We are in complete agreement with Dr. Eliassen's remark that Solberg's stability criterion can easily be established by direct solution of the perturbation equations and does not depend on the ' particle method ' of approach. No doubt it would have been better if we had said so explicitly (and we certainly did not intend to imply the contrary) but we were concerned rather to provide some sort of introduction for non-specialists and a complete deductive treatment would have been outside the scope of our article.

With regard to the restriction of Solberg's stability criterion to axially-symmetric meridional circulations it is worth pointing out that there is certainly no restriction to *meridional* circulations. Whether the destabilising of a current is due to lateral or vertical shear the same stability criterion applies whatever the direction of the shear. For example a sufficiently large E.-W. gradient of velocity or a sufficiently strong N.-S. thermal wind both involve instability according to a criterion identical to that applicable to meridional circulations. The theory therefore *does* apply to all *uniform* circulations in a vertical plane, i.e., for equal rates of overturning in parallel planes. If the rate of overturning is supposed non-uniform we are faced with a much more complicated problem and no precise statement can be made until this problem has been solved. It is by no means evident that in this case there *exist* perturbation solutions which grow without structural change. In order to extend stability theory to deal with disturbances which do not conserve their structure (i.e., ' shape ') it is necessary to define

ore precisely what we mean by instability. The natural definition is in terms of the kinetic nergy of the disturbance and we might define any disturbance to be unstable if its kinetic nergy increases with the time. (This is certainly consistent with the definition of stability elative to disturbances of constant structure). However this definition is less attractive an appears at first sight. In an analogous problem one of us (E.T.E.*) found a great many isturbances of variable structure which according to this definition would be called unstable. lowever further investigation showed that the growth was only temporary, the disturbances ubsequently decreasing in energy. Only in the case of disturbances of constant structure an we infer without further investigation that they will grow indefinitely or rather until the eglected non-linear terms take effect.

Dr. Eliassen states that there are nevertheless reasons to believe that the stability proper-es for axially-symmetrical perturbations are not unimportant for the growth of wave dis-urbances. This is certainly true in the sense that stability properties for different types of isturbance are not unrelated. For example, a decrease in stability for axially-symmetrical erturbations is usually accompanied by an increase in instability for (non-symmetrical) aroclinic waves.

A proof of the statements made above would require a rather lengthy article and we may erhaps be excused on the grounds that our article was intended only as a guide to some of ne simpler new developments.

mperial College, London ;
leteorological Office, Dunstable.
1 November 1951

Notice

The Napier Shaw Library, University of Cambridge

Before leaving Cambridge in 1901 to become Secretary of the Meteorological Council nd, later, Director of the Meteorological Office, Sir Napier Shaw had been for over 20 years Tutor of Emmanuel College and a Lecturer in Experimental Physics at the Cavendish aboratory. He retained throughout his life his affection for his old University and maintained is ties with it. In his will he bequeathed to the University his extensive meteorological brary and also directed that the bulk of the residue of his estate should be used ' to make rovision for the development of the study of the atmosphere in the University of Cambridge,' oping thereby to further a project which had long been in his mind.

The bequest has taken the form of a generous endowment for the library, which has een housed in a pleasant, well-lit room in the Cavendish Laboratory, with accommodation or about fourteen readers or workers. It forms an invaluable aid and stimulus to workers t that Laboratory, which has become the main centre for meteorological research in Cambridge, nd whose staff now includes a Lectureship in Meteorological Physics. The central position f the library also makes it a natural centre and focus for workers in related fields in various ther neighbouring scientific departments.

On the occasion of the Cavendish Laboratory Open Day, on 11 June 1951, the library as formally declared open by the Vice-Chancellor who expressed the University's pleasure nd gratitude for the generous bequest. The pleasant ceremony was attended by a number f distinguished meteorologists, by representatives of the University and by friends of r Napier Shaw.

T.W.W.

* Precisely because this investigation did not appear to lead anywhere it was not published. Mention of it will however be und in records of colloquia given at Geofysisk Institutt, Bergen in April 1947.

Elections to membership and proceedings at meetings of the Society

The following candidates were elected by Council during the Summer recess in accordance with By-law 18 :

Fellows

Allen, Herbert Arthur, Merrion House, Lower Fitzwilliam Street, Dublin.
Andermann, Alfred, B.Sc., 94 Highfield Lane, Southampton.
Carruthers, Inst. Lt. R.N., John, B.Sc., Sea View, Port Carlisle, Carlisle.
Cruickshank, Douglas, 63 Amberley Gardens, Stoneleigh, Ewell, Surrey.
George, John, Castle Bridge House, North Warnborough, Nr. Basingstoke.
Giles, Stanley George, Senior Operations Officer, Far East Area, B.O.A.C. Kai Tak Kowloon, Hong Kong.
Holl, Manfred Mathias, District Aviation Forecast Officer, Trans-Atlantic Bldg., Montreal Airport, Dorval, Quebec.
Huckins, H. G. U., 36 Rose Street, Birchgrove, Sydney, N.S.W.
Hung, Kenneth Roland, Technical Officer, Royal Observatory, Kowloon, Hong Kong.
Livesey, Harold, B.A., B.Com., 23 Wilworth Crescent, Blackburn, Lancs.
Rignall, George Desmond Antony, Far East Flying Training School Ltd., Kai Tak Airport, Hong Kong.
Templeton, H., M.O.10., Air Ministry, Kingsway, London, W.C.2.

Foreign Members

Chou En-Tsi, Royal Observatory, Kowloon, Hong Kong.
Giles, Keith C., M.A., 305 San Miguel Way, Sacramento 16, California.
Tin-aung, George, Air Traffic Control Officer, 85-87, 123rd St., Rangoon, Burma.

Associates

Boyd, Donald William, Dept. of Transport, Goose Airport, Labrador, Canada.
Bristow, Glen Ellis, Kalwood, Man., Canada.
Brunning, Frank Joseph, 315 Bloor Street, W., Toronto, Ontario, Canada.
Busche, George Matthew, 3116, 2nd Street, N.E. Calgary, Alta., Canada.
Dawes, Miss Dorothy Frances, 315 Bloor Street, W., Toronto, Ontario, Canada.
Edwards, Sqn./Ldr. James Francis, R.C.A.F. Station, St. Hubert, P.Q., Canada.
Fraser, David Bann, 315 Bloor Street, W., Toronto, Ontario, Canada.
Hake, Richard Donald, Meteorological Office, Malton Airport, Malton, Ontario, Canada.
Howe, Edward Alexander, 315 Bloor Street, W., Toronto, Ontario, Canada.
Inch, George Donald, 315 Bloor Street, W., Toronto, Ontario, Canada.
Lewis, William Garwood, 315 Bloor Street, W., Toronto, Ontario, Canada.
Luebke, Allan Charles, 315 Bloor Street, W., Toronto, Ontario, Canada.
MacKenzie, Sqn. Ldr., Andrew Robert, C.O. 441 (F) Squadron. R.C.A.F. Station, St. Hubert P.Q., Canada.
McMorran, John Francis, 315 Bloor Street, W., Toronto, Ontario, Canada.
McPherson, George Anderson, 315 Bloor Street, W., Toronto, Ontario, Canada.
Mateer, Carlton Livingstone, 315 Bloor Street, W., Toronto, Ontario, Canada.
Meeres, Leonard Stewart, 315 Bloor Street, W., Toronto, Ontario, Canada.
Quellet, Alcide, 315 Bloor Street, W., Toronto, Ontario, Canada.
Quine, John Frederick, Aviation Forecast Office, Municipal Airport, Calgary, Alta., Canada.
Reinelt, Erhard Rudolph, 315 Bloor Street, W., Toronto, Ontario, Canada.
Shenfield, Louis, 315 Bloor Street, W., Toronto, Ontario, Canada.
Stead, Clifford Jackson, 315 Bloor Street, W., Toronto, Canada.
Vockeroth, Robert Ernest, 315 Bloor Street, W., Toronto, Ontario, Canada.

Student Member

Graybow, Michael J., F.R.G.S., 55 Middleway, Hampstead Garden Suburb, N.W.11.

The following candidates were elected by the Council on 17 October 1951 :

Fellows

LYRES, KENNETH JOSEPH, 52a West Wycombe Road, High Wycombe, Bucks.
IGGS, ARTHUR, 39 Denness Road, Sandown, I. of W.
OWEN, EDWARD GEORGE, O.B.E., Ph.D., M.Sc., Chief Officer, Division of Radiophysics, C.S.I.R.O., Sydney, Australia.
ELAND, RAYMOND JOHN, B.Sc., c/o Meteorological Branch, Box 1289 K, G.P.O., Melbourne, Australia.
YMOTT, DAVID KENNETH, Wey Meadow, Bell Road, Haslemere, Surrey.
INCH, HERBERT F., F.R.A.S., Superintendent, Magnetic and Meteorological Dept., Royal Greenwich Observatory, Herstmonceux, Sussex.
OBIE, CHARLES HENRY, B.Sc., F.R.G.S., Bulkeley Place, Beaumaris, Anglesey.
EARN, ROBERT EDWARD, B.Sc., 91 Ranelagh Road, Felixstowe, Suffolk.
ENKINSON, ARTHUR FREDERICK, B.A., 9 Whitefriars Drive, Harrow Weald, Harrow, Middlesex.
AMBLEY, ARTHUR, 34 Cockington Road, Bilborough, Notts.
ROTHEROE, Flt. Lt., ELMER, D.F.C., Officers' Mess, R.A.F. Kasfareet, M.E.A.F.15.
EARLE, SIDNEY ALEXANDER, Martin's Five, Birdham, Chichester, Sussex.
MITH, FREDERICK JOHN, 4 Chapel Street, Waterbeach, Nr. Cambridge.
TRINGER, EDWARD TREVOR, B.Sc., Meteorological Office (M.O.18), Headstone Drive, Harrow, Middlesex.
AYLOR, JAMES A., B.A., M.A., University College of Wales, Aberystwyth.

Foreign Members

EALL, JAMES M., Meteorological Consultant, U.S. Embassy, Athens, Greece.
ELMONT, ARTHUR D., M.S., Dept of Meteorology, University of California, Los Angeles, 24, California, U.S.A.
EVIAN, VAHÉ JOSEPH, Irrigation Directorate, Baghdad, Iraq.

Associates

AMMOND, DENIS MALCOLM REDVERS, 510 Grosvenor Street, London, Ontario, Canada.
ORNAN, L.A.C., JOHN F., MPO, R.C.A.F., 1015, Fort Churchill, Man., Canada.
ESHIER, JOHN ROBERT, MPO, 1015 (R.C.A.F.) Fort Churchill, Man., Canada.
ILLIAMS, EDMUND DWIGHT MACKENZIE, c/o D.A.F.O. Sea Island Airport, Vancouver, A.M.F., B.C., Canada.

Student Members

AWSON, MICHAEL HENRY, St. Helier, Brokes Road, Reigate, Surrey.
ALLETT, JOHN, 719 Wells Road, Bristol 4.
NGAMELLS, KENNETH ROY, 16 Royce Road, Spalding, Lincs.
IPER, CHRISTOPHER PETER, 19 Lower Fairview, Dartmouth, Devon.
MITH, RALPH, 21 Upper Bridge Road, Chelmsford, Essex.
UCKER, GILBERT BRIAN, 13 Ryecroft Street, Fulham, London, S.W.6.

First Annual Dinner, 17 October 1951

The first Annual Dinner of the Society was held on 17 October 1951 at the Connaught ooms, London. Some 60 members and guests were present, the newly-elected President, ir Charles Normand, C.I.E., being in the chair. The guest-of-honour was Marshall of the ir Force, Sir John Slessor, K.C.B., Chief of Air Staff, who proposed the toast of the Society, nd the response was made by the retiring President, Sir Robert Watson-Watt, C.B. A full ccount of the proceedings was published in the November 1951 issue of *Weather*.

Ordinary Meeting, 21 November 1951

The following candidates were declared to have been elected that day by the Council :

Fellows

Ascough, John Cecil, B.Sc., 9 The Bank, Barnard Castle, Co. Durham.
Brown, Paul Rodham, M.Sc., A.R.C.S., D.I.C., Meteorological Office (M.O.1.), Air Ministry, Headstone Drive, Harrow, Middlesex.
Burling, Ronald Wm., M.Sc., 1 Greenwood Road, Thames Ditton, Surrey.
Cameron, Angus John Munro, Cathay Pacific Airways, 1 Connaught Road Central, Hong Kong.
Campbell, Alexander Bower, 2 Grove Road, Stratford-on-Avon, Warwicks.
French, Donald Peter, B.Sc., 24 Columbia Avenue, Sutton-in-Ashfield, Notts.
Gallagher, Sqn. Ldr., Trevor Alfred, Royal Air Force, Thornhill Station, Gwelo, Southern Rhodesia.
Gregory, Stanley, B.A., 24 Prescot Drive, Liverpool 6.
Harding, Alexander George, M.A., Park House, Frampton Cotterell, Nr. Bristol.
Kerr, Donald, Department of Geography, University of Toronto, Toronto, Ontario, Canada.
MacBeath, Donald Hamish, 24 Youngs Rise, Welwyn Garden City, Herts.
Packer, Robert Wilfred, 47 Hale Street, London, Ontario, Canada.
Siddons, Frederick William Cecil, 37 Kitchener Road, Thornton Heath, Surrey.
Vivian, Jack, D.S.O., D.F.C., 11 Bournemead Way, Northolt, Middlesex.
Wood, Kenneth, B.Sc., 33 Woodlands Avenue, Eastcote, Ruislip, Middlesex.

Foreign Members

Schallert, William, B.S., Department of Meteorology and Climatology, University of Washington, Seattle, U.S.A.
Spar, Jerome, Ph.D., New York University, College of Engineering, University Heights, New York 53, New York, U.S.A.

Associates

Bird, John Brian, Department of Geography, McGill University, Montreal, P.Q., Canada.
Chapman, John Doneric, Department of Geology and Geography, University of British Columbia, Vancouver, B.C., Canada.
Montgomery, Miss Margaret Robinson, 21 Cooper Street, Apartment 6, Ottawa, Ontario, Canada.
Wahl, Herbert Ernst, Box 137, Whitehorse, Yukon, Canada.
Lafinhan, John B. P., 16 Tenby Avenue, Withington, Manchester, 20 (temporary address).

Student Members

Apperley, Eric Charles, Dunvagan, Newby West, Carlisle, Cumberland.
Calder, James Robert, 41 Bladindon Drive, Bexley, Kent.
Johnson, Russell Farrington, 3 School Lane, Thorpe St. Andrews, Norwich.
Pothecary, Ivan J. W., B.Sc., 55 Forest Road, Melksham, Wilts.

The following papers were read and discussed :

(a) ' A study of cold fronts over the British Isles ' by H. W. Sansom (*Q.J.*, **77**, p. 96).

(b) ' The apparent diurnal temperature variation in the lower stratosphere ' by R. H. Kay (*Q.J.*, **77**, p. 427).

Reviews

ugh Robert Mill : an autobiography. London (Longmans, Green). 1951. 8vo. Pp. xii, 224 ; 6 plates. 18*s*.

This is a book of outstanding merit and interest. It portrays in masterly style and with all modesty e life of a man who waged through nine decades a brave struggle against wretched health and bodily firmity, to which was added later a failure of eyesight culminating in total blindness. As a child, Mill as for seven years wholly incapacitated by a complication of tubercular troubles. Having survived these, eyond expectation, he was dogged during manhood by a train of illnesses which sometimes brought m near to death's door. But from generations of tough Scottish ancestry he had inherited the motto No surrender.' He was inspired with the will to live and, moreover, to live long and usefully. This etermination carried him triumphantly through his repeated battles with the outrages of fortune. On p of his physical disabilities he had at the outset of his career to encounter poverty as well. Neglect e was never called upon to suffer ; his extraordinary abilities appear to have been recognized from the e first.

Mill's original intention was to entitle the work under review *My nine lives*. When the time for ublication drew near he thought better of this because, he says in his preface, he had not lived ' in reality ne lives, but one life composed of a number of closely-woven strands, like the log-line used for a patent g to measure the speed of a ship, not merely twisted together loosely like a piece of string.' Nevertheless, s first choice of title could have been well justified, for he possessed an exceptional diversity of interests nd was professionally engaged at one period or another in at least nine distinct occupations. His prowess several of these earned him fame that extended far beyond his native land. He was a noted pioneer in tablishing the modern academic basis of geography, an authority of international repute on polar explora- on, and a widely-followed leader in the scientific study of rainfall. This Society elected him to presi- ency in 1907-09, made him its Symons Medallist in 1918, and just before his death in 1950 chose him first recipient of the biennial award newly instituted in his honour and named after himself. During s early days Mill achieved distinction as an analytical chemist and hydrographer ; he was for a time a niversity lecturer and later became librarian to the Royal Geographical Society, an assiduous author nd editor, a biographer, a journalist, and, occasionally, an inventor, a broadcaster and a poet. He guided e destinies of the British Rainfall Organization as its helmsman from 1903 until the Meteorological ffice absorbed it in 1919.

All these and other activities are either described or touched upon in the autobiography. Its sixth apter, *The distribution of rain*, is an amplified version of an article entitled *Meteorological memories* hich Dr. Mill contributed at the invitation of Sir George Simpson, then the Society's president, to the ctober 1941 issue of this *Journal* by way of marking the author's 80th birthday and 50th year of Fellow- ip in the Society.

The book is no mere formal record of an amazingly versatile career ; it is a human document based the main on a diary which Mill succeeded in keeping without a day's interruption from the age of 5 until shortly before he died towards the close of his 89th year. We are told of his nursery memories, his joys and sorrows, hopes and fears, frequent successes and rare failures through youth and manhood, his two marriages – the second of them at the age of 76 with his secretary Alfreda Dransfield – of his timate friends, among whom were Nansen and Shackleton, and of his seemingly innumerable acquain- nces, which included such men as J. M. Barrie, Arthur Conan Doyle and James Frazer, as well as most the leading lights in the scientific world. We learn, incidentally, that the most northerly cape dis- vered in Franz Josef Land by F. G. Jackson during the 1890's was named after Mill, thus giving him e immortality of the atlas.

Gratitude is due to Professor L. Dudley Stamp, through whose good offices this fascinating auto- ography was brought to publication, and who provides an introduction and postscript which add aterially to its value.

E.L.H.

eports on progress in Physics, Volume XIV (1951). London (Physical Society), 1951. 8vo. Pp. 412 ; plates, Figs. £2. 10*s*.

The *Reports on progress in Physics*, published each year by the Physical Society, provide the physicist th a means of keeping in touch with the latest developments in fields of physics which he cannot hope follow by reading, *in extenso*, the vast amount of published work which now appears in the many urnals. The reports never fail to provide interest, for even in one's own field it is exciting to read a view of recent progress which enables one to see the advances from someone else's point of view. The esent volume maintains the high standard which previous numbers have set and it is to be recommended all who profess and call themselves physicists or physical meteorologists ; some parts ' to be tasted, ners to be swallowed and some few to be chewed and digested.'

There are eleven articles and, as is customary, the last article is on some branch of applied physics. is time the subject chosen is ' Some applications of nuclear physics in medicine,' and this article should ' tasted,' if only to show that nuclear physics is not devoted solely to the production of bigger and

better atom bombs. There are seven other articles on specialised branches of physics which will be of varying interest to meteorologists ; for example, the article by W. C. Price on ' Ultra-violet absorption spectroscopy ' should be read by those interested in the measurement of atmospheric ozone. The quantity of ozone in the atmosphere is measured by means of ultra-violet absorption techniques and Dobson and his collaborators led – and still lead – the way in this field. Their specialised techniques are, in fact, of general application and deserve more than the passing comment which they receive in this review.

Those articles which readers of the *Quarterly Journal* will wish to ' swallow ' are the articles : ' The spectrum of the airglow and the aurora ' by A. B. Meinel and ' A critical survey of ionosphere temperatures ' by N. C. Gerson. Both the articles relate to regions of the atmosphere beyond ordinary reach and for which we are dependent upon indirect methods of investigation for our knowledge. The first article is an account of the very large number of observations which have now been made upon the light which originates from the high atmosphere, either as a steady uniform glow or as aurora, and on their interpretation in terms of the conditions there. The second paper, on the ionosphere temperatures, is more theoretical. Far above the clouds the high atmosphere is indeed a mathematician's playground and only those whose mathematics is polished bright by frequent use will make much headway with this paper.

Of most interest to readers of the *Quarterly Journal*, is the article by B. J. Mason and F. H. Ludlam on the ' Microphysics of clouds.' This article is to be ' chewed and digested.' In fifty pages packed with information the authors give an account of most of the latest experimental and theoretical work on all aspects of cloud physics ; from the beginnings of condensation (with, or without, nuclei), to the production of ' rain, hail, sleet, or snow.' The seventy references form a valuable bibliography. To criticize the article would be churlish, but this reviewer hopes that all readers will read it critically. There is the possibility that the reader will be persuaded that all is now understood when this is not so. The authors place too much confidence in theory, and this is very dangerous indeed when even the experimental basis is very doubtful. Meteorology has a long and unhappy history of theories developed prematurely about processes not yet properly investigated experimentally, and there is the possibility that this is occuring in cloud physics. The reader is warned.

A.W.B.

The earth's magnetism. By S. Chapman. London (Methuen), 1951 ; 2nd edition. 8vo. Pp. xi, 127 ; 39 figures. 6*s*.

This excellent little work was first published in 1936 and was reviewed in this *Journal*, **62**, p. 569. The general arrangement of the book and the greater part of the text remain unchanged, with the principal divisions of the subject as before, the main field and secular variations, the transient magnetic variations : the quiet-day solar magnetic variation, the lunar variation, morphology of the magnetic disturbance field and solar relations with magnetic disturbance. There are, however, additions and improvements in detail. Better and more up-to-date world charts (for 1945) of the lines of equal declination and of equal horizontal magnetic intensity replace the 1922 versions in the early edition and the picture is completed with three additional charts, also for 1945 and on the same scale, of the lines of equal vertical intensity, of equal inclination and of equal total intensity.

In the chapter on the quiet-day solar variations rather more prominence is now given to the ' dynamo ' theory ' and a note is added on the (still unexplained) abnormally large daily variation of horizontal force at the magnetic observatory of Huancayo in Peru near the geomagnetic equator.

In the discussion on the morphology of the disturbance field, use is made of considerable additional data, including data from the Second International Polar Year 1932-33, and diagrams are included showing the overhead ionospheric electric current system which could produce the external part of the *D* field.

The book concludes with an addition in the form of ' Suggestions for further reading.' An improved index facilitates the use of the book as a work of reference.

A.H.R.G.

The Oxford Atlas. Ed. by Clinton Lewis and J. D. Campbell. London (Oxford University Press), 1951. D. folio. Pp. 96, xxvi, 88. 30*s*. (School Edition 25*s*.).

This handsome production, characteristic of its publishers, contains, within a stout pair of cloth covers, 8 pages of introductory matter, 88 pages of relief maps, 24 pages of distribution maps (climate, vegetation, structure, etc.), and a gazeteer of 90 pages. It is a new production making use of a new and pleasing system of layer tints, in which the name of the draughtsman of each map has been shown beneath it – a happy return to eighteenth-century practice. Contour lines as such have been omitted from the relief maps ' since they interfere with the smooth gradation of the layers and with the legibility of the lettering ' ; the junctions of the layer tints are however in general quite clearly defined.

The following comments are offered not so much in criticism as for sympathetic consideration by he editors when revision is undertaken : (i) Clearer delineation of ice limits in the polar oceans would e desirable. (2) In an age of road transport, one would like to see more roads on some of the maps. There is apparently no major east-west road through Southampton !). (3) The climatic maps could arry additional desirable information while retaining clarity, e.g., sea temperature, and more wind (or ressure) data.

This is a highly successful atlas within its avowed aims and should find a responsive public. The rice seems not unreasonable for the content and form.

P.A.S.

eylon. By Elsie K. Cook, revised by K. Kularatnam, London (MacMillan), 1951. 8vo. Pp. 360 ; 122 illustrations, maps and diagrams. 12*s.* 6*d.*

This book was written, following a year's visit to Ceylon, primarily to provide a text-book about the eography, resources and people of the country for use by Ceylonese students. It is clear that Elsie Cook's tudy of the island was extremely comprehensive and penetrating ; she expounds her subject with loving are and there are many signs of her genuine concern for the welfare of the country.

The climate of a country which, although smaller than England, can have in some places more rain one day than London receives in a year, whilst in others the country is quite barren, is bound to be f great interest, indeed exciting. In the section on climate the author takes pains to explain first the osition of Ceylon in relation to the great monsoonal systems of the Indian Ocean area, before plunging nto climatological statistics. The variation of rainfall and cloud over the island then follows logically rom the monsoonal sequence, when considered in relation to the substantial orographic features of the land and the normal diurnal variation to be expected in the tropics. An interesting point which is, risely, stressed is that a rainfall of from 25 to 50 in. a year, which is more than sufficient for full agri- ultural development in temperate latitudes, supports only sparse scrub in Ceylon owing mainly to the ery high rate of evaporation after convective tropical showers. Furthermore, even a rainfall of from 0 to 75 in. a year permits only very limited agriculture except in association with irrigation. The meteoro- ogist will note the absence of any synoptic treatment or examples but the latter is difficult for a tropical rea and would have been inappropriate in a book of this character.

There is an interesting chapter on climatic influences on man, dealing partly with the enervating ffects of even high temperature and humidity on Europeans, but mainly with the influence of climate n the racial characteristics of the Ceylonese themselves.

In the remainder of the book, we hear of the fantastic pilgrimages by early travellers to Ceylon, ne profound effects of the series of European conquests, and a full account of the geology, vegetation, nimal life, etc., of the island. Throughout, interest is maintained by an abundance of entertaining facts, s for example that the word ' cocoa,' in place of ' cacao,' springs from a perpetuation of a spelling error n an advertisement, that insect life is about 100 times more prolific than in temperate latitudes, and so on.

The author is at great pains to direct attention to the dominating influence exercised by the climate ver every facet of life in Ceylon, and the specialist meteorologist, concerned with other fields of his ubject, will be impressed by the innumerable examples of these effects which occur throughout the book.

G.A.C.

eport of the Desert Locust Survey, Oct. 1948/Sept. 1950. Nairobi (Government Printer). 1951, 8vo. Pp. 45 ; 13 Figs., 2 maps. No price given.

This is the first published *Report of the Desert Locust Survey* which was formed in October 1948, ith headquarters in Nairobi, Kenya, with the object of solving the problem of the desert locust in the Iiddle East and in East Africa. Starting with the nucleus of locust-control organizations formed during ne war with headquarters in Cairo and in Nairobi, the new Desert Locust Survey was soon faced with ıortage of both staff and equipment during a period when it had to meet the menace of another locust vasion. That its efforts were so successful and that at the end of the period the Survey had acquired much knowledge of locust behaviour is a magnificent tribute to the work of all those concerned.

This report gives refreshing evidence of how successful international co-operation can be in a field this nature, since locusts, like the weather, take no account of political boundaries, and the account of perations over very difficult terrain in little-known areas is most interesting.

While the report does not treat technical aspects of the work of the Survey in detail, meteorologists ill note what a considerable influence weather conditions have on the behaviour of locusts, and it would ppear that locust movements, at least once they are in the air, are almost entirely governed by the air ırrents in which they are flying.

Studies are proceeding both on the microclimate and on the relation of locust movement to synoptic meteorology ; from the work so far done on these subjects it is clear that this is another fascinating aspect of applied meteorology, which gives promise of enabling the Locust Survey to forecast both the time of an outbreak and the subsequent movements of the locusts. As the Survey has now been relieved of the strain of carrying out the actual control measures, more research will be possible, and meteorologists generally will look forward to seeing accounts of it.

W.A.G.

Annual Report of the British West African Meteorological Services, 1949. Lagos (Government Printer), 1951. 8vo. Pp. 18. 9*d*.

This annual report is the first to be published since 1938 and considerable changes in the organization have been made since then. The Meteorological Service was founded in 1937 and it was on a small scale prior to 1940. During the war period considerable development occurred to meet the requirements of aircraft flying across the Atlantic and to the Nile Delta and elsewhere. A new organization was brought into being in 1947 and comprises a Regional H.Q. at Accra and a local service in each of the four colonies : Gambia, Gold Coast, Nigeria and Sierra Leone. There were in 1949 a total of 270 stations comprising a group, main, and subsidiary synoptic stations, and agricultural, climatological and rainfall stations, with the larger number in the Gold Coast and Nigeria. The report provides details of the functions, staffing and administration of the services.

R.S.R.

Handbook on Aerosols. Chapters from the Summary Technical Report of Division 10, National Defence Research Committee, selected and published by the United States Atomic Energy Commission. Washington, D.C., 1950. 4to. Pp. 147 ; numerous figures and tables. No price given.

This book is a selection of material, appropriate for physicists at work on aerosols. It consists of eleven self-contained chapters entitled :

(1) The absorption wave (a study of the various factors which determine the variation in concentration of a toxic gas effluent from a bed of charcoal) ; (2) General meteorological principles ; (3) Micrometeorological instruments ; (4) General properties of aerosols ; (5) Stability of aerosols and behaviour of aerosol particles ; (6) Formation of aerosols ; (7) Optical properties of aerosols ; (8) Measurement of particle size and size distribution ; (9) Filtration of aerosols ; (10) Methods of testing smoke filters ; (11) Travel and persistence of aerosol clouds.

As could be expected, there is a great variation in standard of treatment, and the advanced meteorologist will be chiefly interested in 2, 3 and 5. Chapters 4, 6 and 7 are mainly physics, and 1, 8, 9 and 10 can best be described, perhaps, as gas and smoke technology.

The student of micrometeorology will find in Chap. 2 a succinct account of M. D. Thomas's work at Salt Lake City, involving continuous observations of wind and temperature at a series of heights up to 440 ft. This is followed by a summary of what is known of micrometeorology in wooded areas, with reference to woods in Florida and jungle near San José. Inversions are frequent below the canopy, both by night and by day, and katabatic winds are fairly common. Although penetrating sunshine is an important factor in breaking up inversions, a turbulent wind above the forest also has an effect. This is not due merely to turbulence caused by the roughness of the canopy, for there is a correlation between the temperature gradient or wind velocity within a forest and the same quantities measured in a field nearby.

Chapter 3 contains a discussion of anemometers and temperature apparatus, and a tribute to A. C. Best's vane for gustiness. There are some useful hints on recording apparatus, including the trick of passing a small alternating current through a D.C. recorder to keep the pen mobile. Students might enjoy making and using the smoke-puffer bottle, for producing puffs of titanium chloride smoke at a distance and enabling winds to be observed directly.

A section in Chap. 5 deals with coagulation by sonic and supersonic vibrations. Experimental evidence is given that visibility in a natural water fog can be increased from 200 ft to some 300 to 400 ft by these means, but there is no evidence that they are useful in higher visibilities. Road traffic in a London fog might be kept moving (possibly to the accompaniment of a very loud noise) but there is no hint that the same methods could be successfully applied at London Airport.

The other chapters are of a less meteorological interest, except Chap. 11, and this surely cannot be taken as representing all that is known on its subject. The suggestion that smoke trails from chimneys occasionally fall to the ground after some travel, because of absorption and radiation by the carbon dioxide they contain, is novel, and will require looking into.

A.R.M.

oceedings of the first international meeting on alpine meteorology. Milan and Turin, 20/30 September 1950. Milan (Instituto Geofisico Italiano). 1951. 8vo. Pp. ii., 165 ; Figs., tables. No price given.

The publication under review contains the speeches and 22 papers given at the meeting indicated the title, with brief reports of the discussions. The speakers were from various European countries d the United States.

Prof. M. Bossolasco, Director of the Institute, in opening the meeting dwelt on the need for close laboration between meteorological services in the alpine area and on the desirability of an extension the meteorological observing network in the higher Alps as providing the only means of obtaining ntinuous and accurate information on the state of the higher atmosphere.

The papers covered a wide range from Dr. Fultz's description of his researches with models into the w of rotating fluid round obstacles to the selection of sites for towns in mountainous areas. Only a rsory survey of the wealth of information can be given in the space available for this review.

The classic alpine phenomenon, the föhn, was well to the fore. Prof. Bossolasco presented a long paper the relatively neglected north föhn of the southern valleys giving frequencies of occurrence and des- bing individual occasions in detail. A particularly interesting part of his discourse dealt with the lapse e which has about the dry-adiabatic value and may exceed it. Sig. Thams showed that the highest solation values occurred in Ticino during north föhn. Dr. Frey discussed an intense south föhn to ow especially the effect of the solenoidal field over the Alps in the initial development of the south nd in the valleys. Dr. Berg described the remarkable and unexplained physical and mental illness many persons during south föhn, an illness not experienced in the southern valleys during north föhn.

Dr. Schneider-Carius applied his scheme of stratification of the atmosphere to show, in particular, e relation of the snow line to the inversions of the peplopause*, and Dr. Ekhart gave a tentative vertical d horizontal classification of the annual variation of meteorological elements in the Alps.

With a little surprise we find an analysis by Dr. Hovmöller of the radar wind observations at 300 mb d 100 mb over Lerwick and Larkhill. The mean velocity in winter is nearly the same at both levels t in summer it decreases with height and at 100 mb is only about half the 300 mb value. The day- day variations in wind at Lerwick are much less pronounced at 100 mb than at 300 mb. No application alpine meteorology, as such, is made.

The snow and ice of the Alps are the concern of seven papers. With the aid of a very close network observations from the Grossglockner pass Dr. Steinhauser showed how greatly snow depth can vary er short distances. Prof. Mercanton gave a history of glaciological work in Switzerland and Dr. Vanni owed that the retreat of the alpine glaciers during the past 25 years is due more to decreased winter owfall than to increased summer temperatures.

In another paper Dr. Vanni complained of reduction in the effort devoted to meteorological observa- ns in the high Alps and made proposals for the establishment of observing posts at the large number hotels and shelters now open throughout the year. More observations are needed, in particular, for e study of the present climatic change.

On the question of housing in the Alps an architect, Sig. Vinaccia, pointed out that the majority present-day inhabitants of the Alps were compelled to live in towns sited for industrial reasons and not, the old-time farmhouses usually were, primarily for maximum sunshine and shelter from wind. dustrial reasons too often led to building in deep valleys where there was little sunshine. He considered urgently necessary to educate large organizations responsible for planning industrial developments d proposed the establishment of an institute to study the climatology of towns and make observations d experiments, with, for example, plastic topographic models illuminated to simulate sunshine, for the idance of planners.

Other speakers dealt with the strong wind which blows down the Rhône valley to Lake Geneva en there are thunderstorms over the upper parts of the valley and with the relation between geological ucture and lightning strokes.

At its final session the meeting passed resolutions pressing the Italian National Electricity Authority establish meteorological posts at its hydro-electric stations and expressing the hope that the Monte sa observatories would soon be put again into a state of full efficiency.

G.A.B.

uthern Rhodesia rainfall Handbook. Prepared by the Department of Meteorological Services. Rhodesia (Government Printer), 1951. Folio. Pp. 30 ; 13 figures, charts. 7*s.* 6*d.* With Supplement No. 1. *Totals of monthly and annual rainfall for stations in the eastern area.* Pp. 55. 2*s.* 6*d.*

From 1896-7 until the outbreak of the second world war rainfall records for S. Rhodesia were pub- ed in the Annual Report of the British South Africa Company on the administration of Rhodesia. ese were interrupted by the war and it has now been decided to publish current meteorological data in

* The peplopause in Schneider-Carius's scheme of tropospheric stratification ('Typen der Grundschicht,' *Geofisica Pura pplicata*, XII, 1948, p. 4) is the inversion layer normal at a height of 1 to 2 km in anticyclones and forming the upper limit he ground layer (*peplos*, Gk. mantle) of turbulent exchange.

monthly reports and in addition to issue a series of handbooks summarizing the records of temperature, rainfall, etc., over a long period. Rainfall has been given precedence because recent schemes for developing the agricultural resources and mineral wealth of the country are dependent on adequate water supplies and hence on an accurate knowledge of rainfall.

For summarizing rainfall the country is divided into five areas, based on river catchments and the data for each area are to be published as separate Supplements, of which the first is now available.

In the main volume the distribution and general characteristics of the rainfall over the whole of Southern Rhodesia are discussed in considerable detail. The authors have been wise in their general arrangement. There are six chapters : i. General information : ii. General discussion of records : iii. Variability : iv. Variation with distance : v. Incidence of rainfall : vi. Statistical arguments. The first five chapters give a very readable and clear account of the facts, unhampered by technical details ; but the latter are available for those who need them in Chap. vi.

The authors have a happy knack of summing up the results of a statistical investigation in a single easily-remembered sentence, for example : ' Once a wet spell or a dry spell has lasted 3 days, the chances are 3 to 1 that it will persist.' ' The incidence of rainfall at Salisbury could be described as one-third light rain . . . . one-third moderate rain and one-third heavy rain.' Their method of obtaining a measure of the general rain over the Colony is worthy of note as labour-saving without appreciable loss of accuracy.

The volume contains 14 pages of diagrams ; those showing the average monthly and annual rainfall and its variability for a selection of stations in each area are worthy of special mention. The diagram of daily rainfall is also of interest ; had such a diagram been available for this country it would have been easier to confirm or otherwise a statement that the day selected for the 1951 General Election was normally the wettest day of the year.

The volume is an excellent example of reproduction from typescript. The diagrams also are clean and well set out though many could have borne a greater reduction and so avoided the double folding of the sheets. Fig. 11 is somewhat difficult for those unfamiliar with this type of diagram, as neither the abscissa nor the ordinate is identified, and it is difficult to interpret the vertical scale in view of the statement in the text that the frequencies refer to 20 per cent ranges.

The Department of Meteorological Services of Southern Rhodesia is to be congratulated on having made such a good start in summarizing its records.

E.E.A.

Recent additions to the Society's Library

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Obituary

Professor V. F. K. BJERKNES

A correction and an addition

Since I wrote (at rather short notice) the appreciation of the late Prof. V. Bjerknes (*Q.J.*, **77**, p. 520) I have heard the details relating to the death of his younger son during the war. He was a doctor of medicine working in a hospital for tuberculous patients, where he contracted the disease. There can be very little doubt that the main cause of this misfortune was the malnutrition due to the German occupation as he was a very robust person, both physically and mentally.

Some very striking first-hand information about V. Bjerknes' last years is contained in *Bull. Amer. Met. Soc.*, **32**, p. 224. For a man approaching his 90th year his physical and spiritual vitality were quite exceptional. He only had to give up ski-ing in 1948 and he published his last scientific paper in 1949. The notice also comments on the depth of his love for the mountains of Norway.

C.K.M.D.

Mr. N. R. BEERS, M.A., B.S.

Norman Ritner Beers, a Foreign Member of this Society since 22 January 1947, died at a Brooklyn hospital on 27 June 1951 in his 39th year. After graduating at the University of Missouri Mr. Beers came to England as a Rhodes Scholar, specialising in astrophysics at Oxford from 1934 to 1937. On his return to the United States he held the post of college traveller for the McGraw-Hill Book Company until 1941, when he was commissioned in the Naval Reserve. For the next five years he was on active duty, serving successively at the Naval Air Station, Jacksonville, Fla., in the Bureau of Aeronautics, Washington, D.C., and on the faculty of the Post-graduate School of the Naval Academy, Annapolis. On his discharge, with the rank of lieutenant commander, he remained for a time at the Academy as associate professor of aerological engineering ; later he received an appointment at the Brookhaven National Laboratory, where his main concern was with problems involved in the atmospheric dispersion of radio-active waste matter. From 1949 until the onset of his fatal illness compelled him to resign the post on 1 June 1951 he edited the periodical *Nucleonics*.

Beers was joint-author of the McGraw-Hill *Handbook of Meteorology* and contributed a number of papers to technical journals. Uncommon versatility in scientific fields conjoined with engineering abilities of no mean order and boundless mental vigour seemed to have marked him out for a distinguished career. His death at a comparatively early age leaves a gap in the younger ranks of American physicists which will not be found easy to fill.

E.L.H.

Mr. H. D. WILLIAMSON, M.Inst.Mech.E.

We have to record the death, on 14 October 1951, of Henry Dale Williamson, a Life Fellow of the Society, elected 19 April 1905. Mr. Williamson, who resided latterly near Falmouth, started his career as a railway and marine engineer, afterwards becoming concerned principally with the initiation and maintenance of petroleum installations in the Far East. When the 1914-18 war broke out he returned from Japan to England and received an important appointment with the Ministry of Munitions in the Manchester area. On the conclusion of hostilities he set up a consultant engineering business of his own in London, where he remained in practice for the rest of his life.

E.L.H.

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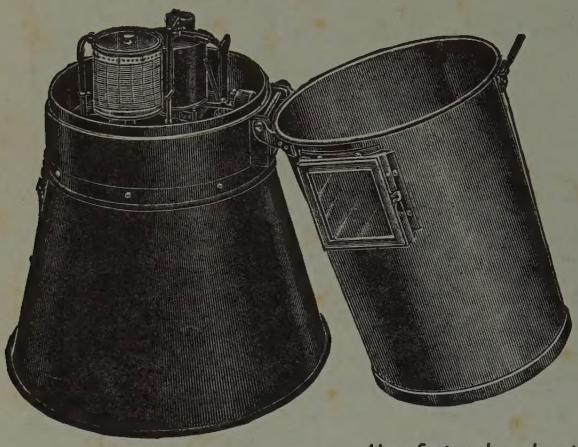

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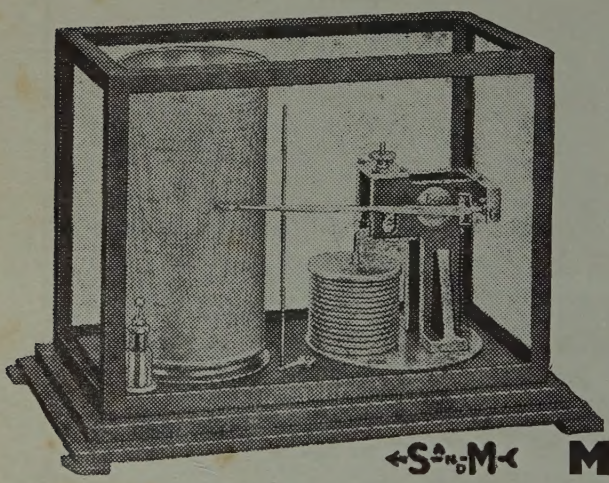

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